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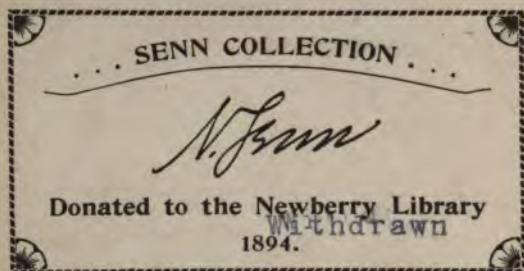
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HALF-YEARLY ABSTRACT
OF THE
MEDICAL SCIENCES.

JANUARY—JUNE.

1865.

THE
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BEING

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Wiener Medizinische Wochenschrift.
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Würzburger Medizinische Zeitschrift.
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HALF-YEARLY ABSTRACT

OF

THE MEDICAL SCIENCES,

ETC.

PART I.

PRACTICAL MEDICINE, PATHOLOGY, & THERAPEUTICS.

SECT. I.—GENERAL QUESTIONS IN MEDICINE

(A) CONCERNING HYGIÈNE.

ART. 1.—*On the Influence of Social Position on Life.*

By Dr. MAJER.

(*Annales d'Hygiène Publique et de Médecine Legale*, Jan. 1865; *Constat's Jahresber.* vii., 58, 1864.)

It is said proverbially that poverty favours longevity, because the poor are not subjected to the bad consequences of luxury and wealth. Dr. Majer shows the folly of the popular dictum. It has been proved by the researches of Benoiston, de Chateauneuf, de Villermé, Casper, and others, that the value of life is less among the impoverished than the rich. Thus, of an equal number of infants of the same age double the number will die of the poorer than of the wealthier class. Where there is the greatest misery, there is the greatest mortality. According to Casper, the mean duration of life among the better classes of Berlin is fifty years, but among the paupers thirty-two years only. The same writer compares the death-rate of the princely and noble houses given in the *Almanach de Gotha*, with the indigent of Berlin, and he shows that of 1000 infants among the former, 57 die in the first five years; but of the same number among the latter, 345. Whilst the half of the poor only have attained the thirty-second year of life, half of the noble have attained the fifty-second. During epidemics the poorer classes are in an especial manner decimated. That simple well-being prolongs life is demonstrated by the low rate of mortality among persons who "assure" their lives in the assurance offices. A fifth or sixth part of Negro slaves die annually. But the mortality among the free negroes who serve in the English coloured regiments is only 3 per cent., that of slaves being 17 per cent. The learned professions, followed generally by persons of easy means, have an incon-

testable influence upon the duration of life. Thus, the mean age of fifty-two French literary men was sixty-nine years. Physicians, according to Dr. Escherich, cannot hope for a long life. At all periods of age they succumb in larger numbers than other professions. But the greatest mortality is during the early periods. Three-fourths die before fifty years of age, and ten-elevenths before sixty. Old men are rare among them.

ART. 2.—*On the Comparative Duration of Life among a Christian and a Jew Population.*

By M. MAYER.

(*Annales d'Hygiène Publique et de Médecine Legale*, Jan. 1865; *Deutsche Zeitschr. für die Statistik*, t. xxi., 1863.)

Dr. Mayer's data are derived from the statistics relative to the Christian and Jew populations of Furth for a period of ten years. The duration of life differs no less than eleven years in favour of the latter population. The mean duration of life among the Christians is twenty-six years, among the Jews, thirty-seven years. The difference, Dr. Mayer thinks, arises from the care, often exaggerated, which the Jews give to their infants, and by means of which they stave off the dangers inherent to the earlier years of life. During the period from one to five years of age, the Jews lose but 10 per cent. of their children, the Christians 14 per cent. The longer duration of life among the Jews is found at every period of life up to the sixtieth year, but after this the relation changes. Thus, from sixty to seventy years, the mortality is from 9·8 per cent. among the Christians, and 12 per cent. among the Jews; from seventy to eighty years it is from 8·9 per cent. for the former, and 7 per cent. for the latter; lastly, from eighty to ninety, it is 2·1 per cent. for the Christians, and 8·1 for the Jews. This is not astonishing, since after sixty years of age, there are more Jews living than Christians. Among the latter, half the persons born living have succumbed at thirty years of age; among the former, not before fifty years. A comparison made between the two races at Frankfort has furnished similar results. In this city, the mean duration of life is thirty-seven years seven months—thirty-six years eleven months for the Christians; forty-eight years nine months for the Jews.

During the first five years of life of 100 Christian infants, 24·1 die; of 100 Jew infants, 12·9. Among the former, 38·1 attain the fiftieth, and 13·4 the seventieth year; among the latter, 54 attain the fiftieth, and 27·4 the seventieth. In short, a fourth of the Christians live only six years and eleven months, while the same proportion of Jews live twenty-eight years three months. Half of the former succumb at thirty-six years of age, of the latter, at fifty-three years one month. There is but a fourth of the Christian population of Frankfort living above fifty-nine years ten months, while a fourth of the Jew population is above seventy-one years.

Dr. Neufoille explains these great differences in the following manner:—At Frankfort there is no proletary among the Jews, whilst a tenth of the Christian population are paupers; the majority of the Jews are commercial or literary men, few among them are operatives, and artisans are not exposed to the same causes of exhaustion. They live also a more regular life. And, although the same conditions of well-being and good conduct belong to the commercial and administrative classes of both religions, it is not less true that of 100 commercial men above twenty years of age, half among the Christians succumb before the fifty-seventh year; but among the Jews, not until the sixty-first year. The great augmentation of the number of Jews is owing to the low rate of mortality among them, and not to the proportion of marriages and births. In Prussia, the Jew population between 1822 and 1840 increased 34·5 per cent., whilst the Christian population, during the same period, increased only 28 per cent. There was 1 birth in every 28 of the former, and in every 25 of the latter; 1 marriage per 139 Jews, and 1 per 112 Christians; 1 death per 40 Jews, and 1 per 34 Christians. At Furth, in an average of 25 years, 1 marriage has been recorded in 128 Christians, and 1 in 149 Jews; 1 birth in 29 Christians, and 1 in 35 Jews. It is worthy of note that in the marriages of the Jews the ages are more advantageous than in those of the Christian population. Thus, of 100 persons, there were but 29 men and 54 women below thirty years of age among the Christians, whilst there were 37 men and 69 women among the Jews. Below twenty-five years of age, the marriages among the former were 26 per cent.; among the latter 45 per cent. The still-born average 1 per 19 Christians, and 1 per 34 Jews. It results from these data that, in regard to the duration of life, the Jews are more favourably situated than the Christians. In fact, as the mean duration of life augments among a people, as in the case of the Jews, so the number of widows and orphans diminishes; the active and productive class is more numerous, and the class of consumers solely is diminished.

ART. 3.—*Researches on Marriages of Consanguinity in the Commune of Batz, near Croisie (Loire Inférieure).*

By Dr. AUGUSTE VOISIN.

(*Gazette Hebd. de Méd. et de Chir.*, Janvier 20 and 27, 1865.)

Dr. Viennois spent a month in Batz, the residents of which have, for centuries, been in the habit of contracting marriages of consanguinity, and of living in nearly total isolation from the neighbouring districts. There were 46 such marriages at the time, in the place. He inquired into the previous history of the husband and of the wife; he examined them and their children, with regard to their physical and intellectual development. He questioned the old members of the community also, and with these various materials

he drew up tables which show that consanguinity has brought on no disease, no degeneration, no arrest of development, and that the stock has remained very handsome and very pure.

ART. 4.—*On Preventive Medicine, as illustrated in the proper use of Food.*

By MR. ERASMUS WILSON.

(*Medical Times and Gazette*, January 27, 1865.)

Mr. Wilson thinks that we are all, especially our children and youths, much under-fed. He recommends three ample meals of mingled animal and vegetable food; and will have “no putting off of the stomach with bread and butter and slop as the effigies of two of the three meals of the day.” Mr. Wilson says:—

“But a period comes when milk is no longer the diet of children, and when custom, originating, as we have seen, in Nature’s promptings, has determined the necessity of three meals in the day. The infant demands more than three meals, and makes no distinction between the day and the night. The day of the infant is a day of twenty-four hours; the day of childhood, as of the remainder of life, has a duration of twelve to sixteen hours. The three meals at present under consideration are the morning meal, the mid-day meal, the evening meal. These meals represent the wants of the body arising during the intervening interval. The morning meal is intended to supply the moderate waste of the night, the mid-day meal the active waste of the morning, the evening meal the active waste of the afternoon. The amount of the three periods of waste is pretty equal; the amount of the supply should be equivalent to that of the waste.

“I am desirous of impressing upon my hearers my opinion and firm conviction that food is not only a necessity, but in civilised life a threefold necessity, and that the three meals should each represent the third of the nourishment of the day, and be so apportioned as to comprehend an equal amount of variety and an equal amount of nourishment. In the primitive life of the labouring class this law is fully appreciated, and is acted upon to the full extent of their means. With the exception of a somewhat more bulky mid-day meal, the morning meal and the evening meal do not far diverge from the standard of the mid-day repast.

“But the educated classes are apt to fancy that they possess a knowledge superior to that of Nature, and the result is a perversion of the law of nourishment that leads to the development of debility and disease. A careful, well-meaning mother, from purest ignorance—another expression for superior knowledge, the “little” knowledge that is so proverbially dangerous—will tell you that she conforms to the law of Nature in providing for her children three meals in the day. She will describe those meals as breakfast, dinner, and tea, and you will find the composition of those meals to be as follows:—A vegetable breakfast, namely, bread and butter,

with tea and a little milk ; a dinner half animal and half vegetable ; and a "tea," vegetable like the breakfast. Here, then, we find education bringing about a total change in the diet of man. Born an animal feeder, he is quickly transformed into a vegetable feeder ; that is, more than two-thirds of his diet is vegetable and the remaining third only animal, the exact opposite of that which I consider should be the standard diet of children, namely, one-third vegetable and two-thirds animal.

"My deduction from these premises is, that children are almost universally under-fed, and that the majority of the diseases of children arise from the debility of constitution induced by this habit of under-feeding. If I am right in this view, preventive medicine may do much in the prevention of disease by correcting an error so widely spread.

"The diet of children of all ages should be, a substantial breakfast, with animal food in some shape ; a substantial dinner of meat, vegetables, and cereal pudding ; and a substantial supper, also consisting, in part, of animal food. The drink may be milk, tea, cocoa, and, possibly, beer. I would call this the diet of health ; a diet capable of making a strong body and also a strong mind ; and a diet capable of preventing disease. Compare it for an instant with the milk-and-water and bread-and-butter diet of some establishments ; the meagre dinner of meat, and the miserable grouting of rice and amylaceous pulp. Rice and amylaceous pulp should have no place in the diet of health, but should be reserved for the sick room.

"Born in prejudice and matured in prejudice, it is the struggle of a lifetime to throw off the trammels of prejudice. We are apt to attach a peculiar signification to the terms which we are in the habit of employing. Ask a person what he usually takes for breakfast, and he will pretty certainly begin his enumeration with the word "tea," the mere drink of the meal ; it is, in truth, with him a mere break-fast, instead of being, as it ought to be, a substantial morning meal. The dinner of labour is the luncheon of fashion ; then follows the mildly alkaline and stimulating drink that is termed "the tea ;" and last of all comes the supper, the late dinner of fashionable life. We have, therefore, before us a succession of three meals and an intermediate drink, but the drink precedes the last meal ; and, therefore, the orderly matron, who is more attentive to her 1, 2, 3 than she is to the intention of the daily fare, prescribes for her children breakfast, dinner, and tea—two slops and a meal. But let her, in good English phrase, call the children's meals breakfast, dinner, and supper, and then we immediately obtain two dinners and one slop, the breakfast—an obvious improvement. I have secured to many a child a reasonable evening meal by suggesting to the mother the mere use of the word "supper" as the name of the third meal. No human being could call bread and butter and tea by the hearty name of supper.

"Assuming that the amount and richness of the supply of food should be determined by the offices which it has to perform, there is no period of life when more food is required than in childhood

and youth. The hard-worked labourer in a long summer's day scarcely exhausts a greater quantity of nutritious matter than a growing boy of ten or twelve years of age; in the labourer the consumption is waste; in the growing boy it is bestowed in the construction of the body, in developing and building up the future man. And it is no uncommon thing to find that although the general construction of the body has been fairly performed, there is some one organ of the economy that has fared less well than the rest, and that part not uncommonly the skin; hence the origin of acne, of the ringworms, *et hoc genus omne*.

"If it be admitted that food is the source of the elements of which the body is composed, what kind of body can be expected in the case of a deficient supply of food, whether that deficiency proceed from actual want or from some perverse theory of refinement founded on a false conception of the nature and objects of food and of its direct convertibility into the flesh and blood of man? Parents are too apt to take their own stomachs as the standard of diet of their children: a cup of tea and a slice of toast suffices for them, so it must suffice for the little ones. I knew a lady who brought up her children on mutton alone, because she herself could digest nothing but mutton. Her children were a feeble, puny, sheepish race, always in the doctor's hands. A mother, in anticipation of the full meal at seven o'clock, can afford a light lunch; but she unfortunately concludes that, because a light mid-day meal is good for her, a spare dinner is equally proper for her children. She has heard somewhere that suppers are heavy and interfere with sleep; so, the children must be content with their tea, and go supperless to bed. Parents have rights over their children, but not the right of feeding them in such a manner as to make them the subject of disease. Such parents become the authors of a puny and degenerate race, and are unintentionally traitors to their country.

"If the two periods of life already adverted to be important in their influence on the future man—namely, the period of infancy, ranging from birth to the age of two years, and the period of childhood, ranging from two years to seven years—the next two periods—namely, those of boyhood and youth—are equally so. While the food of the infant and the food of the child are abundant and regular, the food of the boy and the food of the youth should be the same. Both are occupied in the great business of growing life; on both are dependent the future man, for his strength and for his manhood."

ART. 5.—*On the Jerked Beef, or Charqui, of South America.*

By Dr. —, "Analytical Sanitary Commissioner" of the
Lancet.

(*Lancet*, vol. x., 1865.)

In South America, on extensive pastures, a vast number of cattle are reared, mainly for the sake of their skins or hides, fat, horns,

and hoofs. This number in 1863, according to the best authorities, reached four millions; the surplus unconsumed, and which might be imported annually into this country, provided a ready market existed for its sale, being nearly two millions.

After the removal of the hides, &c., the flesh or muscle free from bone is cut into slices. These are first salted, and then exposed to the action of the sun for a few days until they are sufficiently dried. The slices thus prepared vary in thickness from a quarter to three-quarters of an inch, and in this form they constitute the dried or jerked beef, concerning which so much has been written within the last few weeks.

The beef is, however, prepared in two other forms. In one of these the slices are dried to a much less extent, and put up in rolls. This kind is distinguished by the appellation of moist or rolled beef; whilst in the other the beef is salted in the same manner as our own salted or corned beef.

In order to secure in this country a market for these articles, it is necessary that they should possess three recommendations—first, that they should be cheap; secondly, that they should be nutritious; and thirdly, that they should be free from taint or unpleasant flavour.

The first recommendation, that of cheapness, they certainly possess, each form of the beef being sold at 3d. per lb.; that is, at a little more than one-fourth the price at which fresh meat can be obtained in this country; but then it must be remembered that one pound of the dried beef is equal to about two pounds of lean fresh or undried beef, and therefore its cost scarcely exceeds 1½d. per lb.

Whether they possess the other recommendations adverted to, and are nourishing, wholesome, and free from taint, will be best considered after the results of the analyses to which we have subjected the various samples have been stated. For the sake of comparison, we first give the analyses of English Fresh and Corned Beef.

Sample 1st.

Lean English Fresh Beef.

	Per cent.
Water	73·33
Fat	4·08
Nitrogenous matters, fibrin, albumen, &c.	20·76*
Ash	1·20
Inosite, creatine, and other undetermined matters	·63
	<hr/> 100·00

* Nitrogen 3·197 grs., = 20·76 grs. nitrogenous matters.

Sample 2nd.

Lean English Salted or Corned Beef.

	Per cent.
Water	70·488
Fat	4·374
Nitrogenous matters, fibrin, albumen, &c. ...	18·170*
Ash	6·165
Undetermined matters	·803
	<hr/>
	100·000

Sample 3rd.

Dried Jerked Beef, or Charqui, obtained from Mr. Madden, importer of the preserved meat, 5, Leadenhall-street.

	Per cent.
Water	33·82
Fat	2·958
Nitrogenous matters, fibrin, albumen, &c. ...	40·954†
Ash	20·456
Inosite, creatine, and other undetermined matters	1·812
	<hr/>
	100·000

Sample 4th.

Dried Jerked Beef, or Charqui, obtained from the South American Beef Company, 153, Cheapside.

	Per cent.
Water	29·70
Fat	10·41
Nitrogenous matters, fibrin, albumen, &c. ...	39·69‡
Ash	18·55
Undetermined matters	1·65
	<hr/>
	100·00

It appears from the two preceding analyses—First: that nearly one-third of the so-called dried beef consists of water, namely, 31·76 grains per cent., being the mean of the two analyses; this is therefore considerably less than half the amount found in the sample of English fresh meat examined, which, as we have already seen, contained as much as 73·33 grains. Second: that one of the two samples was very deficient in fat, containing about 3 grains as against 4·08 grains per cent. present in the lean fresh beef. Third: they are particularly rich in nitrogenous matters, as was to be anticipated from the great reduction in the amount of water; the mean result of the two analyses being 40·32 grains, as compared with the 20·76 grains per cent. of the fresh beef, or just double the amount; so

* Nitrogen 2·798 grs., = 18·17 grs. nitrogenous matters.

† Nitrogen 6·307 grs., = 40·954 grs. nitrogenous matters.

‡ Nitrogen 6·113 grs., = 39·69 grs. nitrogenous matters.

that one pound of the dried beef, as respects the nitrogen contained in it, is just equal to two pounds of the fresh lean beef. Fourth and lastly : that the ash of the dried beef amounted to nearly 20 per cent., while in the fresh beef it was only 1·2 per cent. ; this enormous difference being due to the salt with which the beef is impregnated previous to its being dried in the sun.

Sample 5th.

Rolled or Moist Charqui, obtained from the South American Beef Company

	Per cent.
Water	46·69
Fat	7·995
Nitrogenous matters, fibrin, albumen, &c.	27·50*
Ash	16·86
Undetermined matters	·955
	<hr/> 100·000

This beef differs mainly from the dried charqui in the larger quantity of water it contains, which amounts to nearly one half its weight, and which of course entails a proportionate reduction in the quantity of the nitrogenous constituents. The per-centage of water, although large, is still some 26 per cent. below that present in fresh beef.

Sample 6th.

Pickled Charqui, obtained from the South American Beef Company.

	Per cent.
Water	59·00
Fat	12·17
Nitrogenous matters, fibrin, albumen, &c. ...	15·40†
Ash	11·37
Undetermined matters	2·06
	<hr/> 100·00

This analysis of American Corned Beef can scarcely be contrasted with that of English Corned Beef ; for while the latter was made from a sample of lean beef, the former was, on the contrary, from beef containing a considerable proportion of fat. In point of nutriment the two kinds of beef may be regarded as nearly upon an equality ; the amount of salt present in the American beef is, however, double that in the English corned beef. It will be perceived that in both, but more especially the American Pickled Beef, the nitrogen present is much less than in the English Fresh Beef, the difference being due to the salting.

This pickled beef is similar to our own beef in being cut up into joints, and in containing a proportion of bone ; so that, at 3d. a pound, it is not nearly so economical as the dried charqui.

* Nitrogen 4·247 grs., = 27·50 grs. nitrogenous matters.

† Nitrogen 2·371 grs., = 15·40 grs. nitrogenous matters.

The analyses above given are amply sufficient to prove that the several varieties of charqui or jerked beef are highly nutritive; the dried charqui, as compared with English fresh beef, containing about twice as much nitrogenous materials; the rolled charqui, 7 parts more in the 100 than the fresh beef; and the corned beef possessing nearly the same nutritive value as ordinary corned beef. They show also that the quantity of fatty matter in the dried beef is liable to considerable variation, being in the one case 2·958 grains, which is much less than that found in the English fresh beef, and in the other as much as 10·41 grains per cent.

It must not be forgotten, however, that this jerked beef is really not fresh beef at all, but salted beef—a fact which is brought out strikingly in the analyses; and therefore, in any discussions concerning it, it is proper that the article should be regarded from this point of view. In salting the jerked beef, brine escapes from it, in the same way as in salting ordinary fresh beef, and consequently it sustains a loss of a portion of its nutritive materials, as well as of other constituents. This will be rendered evident on comparing the amount of nitrogenous matter contained in the samples of fresh English, and English and South American salted beef.

We now turn to the consideration of the question of the wholesomeness of the beef, and its freedom from taint, and this brings us to treat of the method which should be adopted for cooking the beef.

It is obvious that the sun-dried beef, being deficient in water, and somewhat hardened in the process of drying, and also largely impregnated with salt, requires as a preliminary operation to be well steeped. This abstracts a portion of the salt, causes the meat to swell up greatly, and to acquire increased bulk and weight: a piece of the beef weighing 786 grains, immersed in cold water for eighteen hours, was found on removal to weigh 1109 grains.

This process of steeping, however, tends to withdraw from it a small portion of the nourishment contained in it, for we found in the liquor in which the piece of beef weighing 785 grains was steeped, 42·5 grains of dried nitrogenized matter, chiefly albumen, equal to 5·4 grains per cent., of the 40 per cent. which it originally contained, and 153·5 grains of salt, equal to 14·13 per cent., out of the 18·55 grains present before it was steeped.

Singularly enough, the salted beef yielded to water a larger proportion of nitrogenous matter than the fresh and unsalted beef: thus 1200 grains of the latter treated with successive portions of water, furnished 49·8 grains of nitrogenous matter, equal to 4·16 per cent., while 787 grains of the dried charqui, obtained from the South American Beef Company, gave 66·6 grains, equal to 8·46 per cent. This difference is probably owing to the action of the salt on the meat.

After the steeping, which is the most important part of the process, the meat may be well beaten with a rolling-pin; it must then be slowly cooked, and best by stewing, seasoning of various kinds according to taste being freely used, but no salt.

We have recently been present at an entertainment given by Mr. Madden, at which the charqui was served up in several different forms. The dishes were palatable, and free from any perceptible

taint, with the exception of the stewed beef, and such in fact as no hungry person would decline to partake of. These desirable results are, however, only to be obtained when the meat is in a perfectly fresh state, when care has been taken in preparing it, and sufficient time allowed for the previous soaking. They are most likely to be obtained by using the dried charqui, as the rolled variety contains an amount of water which causes it quickly to spoil when exposed to the trying conditions of warmth and moisture. When not in a perfectly fresh condition, and carefully cooked, the meat possesses a peculiar and somewhat disagreeable flavour, not exactly due to decomposition, and to which meats preserved in tin cases are also to some extent liable.

Owing to the fact of the jerked beef being dried and salted, it is not adapted for roasting, but is best suited for stews, but more especially for soups, and for making potted beef. There are two ways in which the soups may be prepared: one by stewing down the meat and making a strong stock liquor, rejecting the fibrous and undissolved portions; the other by grating the meat up into a coarse powder, adding water, and then stewing. The latter is the easiest method, and the soup produced is the more nourishing, inasmuch as it contains all the constituents of the meat.

In the jerked beef, especially in its dried state, we have, we believe, a very valuable and much desired form of nourishment, for the sale of which a very large field exists in this country. It is necessary, however, that the greatest possible care should be taken in the preparation of the article for this market. We believe that considerable improvement in its preservation might be effected, and that its present condition in this respect is scarcely so satisfactory as is desirable. The consumption of the article in this country is as yet, however, in its infancy, and, doubtless, as the demand for it grows the quality of the imports will improve.

ART. 6.—*On a New Extract of Beef.*

By BARON LIEBIG.

(*Annalen der Chemie und Pharmacie*, Jan. 1865; and *Lancet*, Jan. 28, 1865.)

In an article in the *Annalen der Chemie und Pharmacie* for January, 1865, Baron Liebig describes a new extract of beef which is being prepared in large quantities in Uruguay for consumption in Europe. Since the introduction of this extract of flesh into the Pharmacopœia, its great efficacy in cases of debility, indigestion, &c., has been repeatedly proved; and in order to give an idea of the extent to which it is used, it will be sufficient to state that nearly five thousand pounds of beef are yearly employed in the Court dispensary for its preparation. A great part of this is sold retail (*i.e.*, without a doctor's prescription) in the apothecaries' shops—an undoubted sign that it is employed for household pur-

poses. Even very poor persons who have once experienced its beneficial effects, and who are very much disinclined to spend money on medicine, return to its use of their own accord in the event of illness, notwithstanding its present high price (two shillings per ounce). It is particularly valuable in hospitals, as by its means physicians can prescribe a soup of any required strength, perfectly free from fat. For several years its use has been strongly recommended in the French army by Proust and Parmentier, the latter of whom says: "Dissolved in a glass of wine, it is a powerful restorative, rendering severely wounded soldiers, however weakened by loss of blood, capable of bearing removal to the nearest field hospital." One pound of the extract, boiled with some bread, potatoes, and salt, will make sufficient soup for one hundred and twenty-eight soldiers, and not inferior in strength to that obtained from the best hotels. In fortresses and at sea, where the men are confined to salted and smoked meat, it is the only means of supplying the important ingredients which meat is deprived of in the process of salting. "For the last fifteen years," says Baron Liebig, "I have continually directed the attention of residents in Buenos Ayres and Australia to its preparation, but it is only recently that my efforts have had any sure prospect of realization. In 1862 I received a visit from Herr Giebert, an engineer of Hamburg, who had spent many years in South America and Uruguay, where hundreds of thousands of sheep and oxen are killed solely for the hides and fat. He told me that directly he saw my account of the preparation of this extract he came to Munich, with the intention of learning the process and then returning to South America in order to undertake its manufacture on a large scale. I therefore recommended Herr Giebert to Prof. Pettenkofer, who willingly made him familiar with every detail of the process. He then returned to Uruguay in the summer of 1863; but, owing to the many difficulties which generally hinder the introduction and management of a new business, it was almost a year before he could actually commence the manufacture. Herr Giebert requested permission to call his extract by my name, which I granted; telling him, however, beforehand, that if it contained the least trace of fat, which causes it to become rancid, or the gluey substance, which the ordinary solid broth or *consommé* contains, which predisposes it to become mouldy and entirely deprives the product of the unalterability of the pure extract, I should be the first to publicly assert its inferiority. In return, Dr. Pettenkofer and myself promised to submit each sample to analysis free of cost, and if found genuine to testify to the fact, on condition that he would bring it into commerce at not more than a third of its present price. This arrangement of course relates only to the commencement of the importation, as the testimony of chemists will be no longer necessary when the public are once acquainted with the characteristics of the pure extract. Herr Giebert proposes to produce from five to six thousand pounds per month. The first sample, of about eighty pounds' extract from beef and thirty pounds from mutton, arrived a few days ago in Munich; and we have the satis-

faction of being able to say that for a product from the flesh of half wild animals its quality is excellent ; and we believe that the other conditions—i.e., the price—will also meet our expectations.”

ART. 7.—*On Liebig's Food for Children.*

By MRS. BAINES.

(*Social Science Review*, April 1, 1865.)

“The plan which I have adopted,” says Mrs. Baines, “was suggested to my mind in consequence of the failures that so frequently follow an attempt to bring up infants ‘by hand,’ and the idea upon which I have acted appears to be the same as that which has formed the basis of Professor Liebig’s recipe for ‘children’s soup’—viz., the differences which, by a chemical comparison, are found to exist in the relative proportions of certain elements in cow’s milk and human milk ; and the addition of certain substances to the first-named, in order to bring it up to the standard of woman’s milk.

“In the first place, I will speak of the MALT FLOUR, which is to be added to the wheat flour ‘in order to save the labour that would otherwise be imposed on the organization, by the transformation of the starch into sugar and dextrine in the stomach during the process of digestion, and the change is therefore effected beforehand by the mixture of the two flours.’

“Without waiting to inquire whether the change alluded to does not take place during the process of insalivation rather than by the action of the gastric juice, I would venture to remark, that while care should be taken not to over-tax the digestive organs, it is possible to fall into the error of helping Nature too much ; and although the salivary glands are but imperfectly developed in infancy, yet there is every reason to believe that the infant powers of digestion and assimilation are quite equal to the task of disposing of a description of food that may be made to approximate as nearly as can be in composition to that of the natural aliment ; and that the salivary apparatus, as well as the organs of digestion, will be all the better for being brought judiciously into play at an early period of existence.

“Then, again, with respect to the ALKALI, which, because it is deficient in cow’s milk, Liebig proposes to supply in the form of *carbonate of potash* ; but, let me ask, whether the proportion of wheat flour, which is to be added to the milk to form the ‘soup,’ would not be sufficient to replace the alkali, in order to make the composition correspond in that respect with the mother’s milk ? Bearing in mind that the phosphates and chlorides abound in the cereal above-mentioned : and although the carbonate of potash, in a crystallized form, may be very valuable as a medicinal agent, if required, there can be no question that for dietetic purposes it is better derived from the vegetable kingdom ; the salts which we require in our food are preferable when taken *au naturel* than substituted by the ‘carbonate of potash from the doctor’s shop.’

"As to the *boiling* of milk in preparing infant's food, *practically* I have not found the plan answer; boiled milk can be borne by few; the stomach either rejects it, or if retained, the milk otherwise disagrees; this is my experience. I should like to know in what manner science can account for the fact—for 'fact' it is—the intolerance of boiled milk having occurred in too many instances to admit of an explanation on the score of individual idiosyncrasy. I would merely offer the remark as a suggestion when I say, that it is *possible* the milk, when subjected to the action of a certain degree of heat—a temperature approaching to the boiling point—undergoes a chemical change, which causes the separation of the solids and fluids, and thus the casein becomes coagulable and indigestible; the addition of water to milk is known to cause a separation of the curd from the watery particles, and, for these reasons, I should prefer to mix the water and flour, boil these sufficiently to separate, break, and dissolve the starch granules of the flour, and while still at a high temperature, add, gradually stirring in, the quantity of cold milk sufficient for a meal; sweeten slightly with loaf sugar, and a compound is thus obtained, fulfilling, in all respects, the idea of a model food. As the mode of administering food to an infant is not a less important matter than its preparation, it should be stated that the feeding-bottle ought always to be employed for an infant up to the age of eight or ten months, if not older; 'the act of sucking promotes the flow of saliva, which, mixing with the aliment that is being swallowed, assists digestion.' Liebig, in his article, has omitted making any reference to this important point.

"One word more as to the use of an admixture of the vegetable flour with milk. As cows' milk has an acid reaction, and wheat flour is decidedly alkaline, it is only necessary to point to this fact in order to prove the value of the flour as a *corrective* to the milk, in addition to the other qualities which render it an important adjunct to cows' milk, in the food of hand-reared children."

ART. 8.—*A Suggestion as to the Feeding of Milch Cows, with a view to an improvement in the Constituents of the Milk.*

By "HUMANITAS."

(*The Veterinarian*, 1865.)

"In cows' milk," says Humanitas (Mrs. Baines, we believe), "the sugar and water are less than in human milk; the butter and casein are more, and the salts also are in excess. Cows' milk is not unfrequently acid, especially when the animal is stall-fed, while human milk is decidedly alkaline. Bearing all these differences in mind, it appears to me that every condition indicated might be fulfilled, and that we should obtain a very good balance in the relative proportion of these essential elements in the milk, if we supplied the cow with the kinds of food containing the sugar, the alkali and carbon. Thus by supplementing artificially those constituents—

the milk being naturally rich in butter and casein—we may safely add the proportion of water without unduly impoverishing the whole. * * * * I have not particularised the precise kind of food which it may be desirable to substitute for, or employ in addition to, that which is now usually given as fodder to milch cows. As an *amateur* I cannot be expected to enter into details, having only an instinctive appreciation of the subject to guide me, and a common-sense view of the matter generally. If I did suggest anything, it would be that beetroot or carrots might supply the saccharine properties, ground corn the calorific and the alkali; these, varied according to circumstances, with bran-mashes, grains, or hay, would seem to fulfil all the conditions indicated. It appears rational to suppose that green food, turnips, &c., should be suspended while the experiment is continued; and as the kind of food proposed would come under the description of high-feeding, air and exercise, besides, all other conditions necessary to health should be freely promoted; for it may be presumed that animals undergoing special treatment, can have the advantage of these exceptionally favourable circumstances.”

ART. 9.—*On Muddy Streets from a Sanitary Point of View.*

By Dr. —.

(*Lancet*, March 11, 1865.)

Dr. Farre, in a late “Weekly Return of Births, Deaths, and Causes of Deaths in London,” states the following proposition:—“Given a broad river, with a temperature at the time above that of the air; let there be another vast moisture-exhaling surface on its banks, sixty or more miles in extent, and this area covered with houses which pour smoke from a million chimneys into a still atmosphere, and the result is that almost impervious fuliginous mass called a ‘London fog.’” That we had this problem well worked out a few Saturday evenings back not anyone who witnessed its performance will have forgotten: cough, cold, and bronchitis rose at once in the medical market, and people began their old winter wheeze that night who had not wheezed the whole season before. Though the fog soon left us, the wheezing did not; for the latter when it fastens on its victim is not so readily shaken off as is the yellow palpable air exciting it. But common as fogs are with us, and favoured as we have been this winter with one most unmistakable member of the body, we have been on the whole much freer from them than is usually the case at this time of the year. But if we have had a minimum of fog, we have enjoyed a maximum of mud; for a more sloppy, wet-footing season has not been known for many years. The mud and mess which collected in our streets became not only disgusting, but unwholesome, containing as it did the effete organic matters of such a dense population. Attention was recently drawn by a correspondent (*vide the Lancet of Feb. 18th*,

1865, p. 194) to the fetid exhalations which have arisen from our roadways, covered with a grimy muddy paste of decomposing animal and vegetable matters and inorganic substances. Morning after morning we awoke to the same wretched-looking spectacle, and night after night we returned with our own legs or those of our horses in a most sorry plight. But, as urged by our correspondent, it is not the inconvenience of the dirt alone which is so disagreeable and tiresome. A long continuance of muddy streets is a condition which is most unhealthy. The superincumbent air is kept constantly damp, and noxious matter is unceasingly rising up in it. Pedestrians are not only forced to have damp feet,—a most injurious result to many persons,—but everybody is obliged to breathe abominable emanations. We do not say, of course, that the “highly agglutinative compound, London mud,” is in itself capable of producing typhus fever; but we do think that some of the results it entails may act as predisposing the system, under certain circumstances common more particularly to the poor, to yield to fever-poison germs otherwise generated. At any rate, typhus is again increasing, according to the report mentioned. The Registrar-General observes: “The increase of typhus is a matter of much regret. The medical officers of health have to contend, not only with the evils which attend rapidly increasing populations, but also with the overcrowding of particular localities by the clearing of neighbourhoods for improvements and extensive railway works.”

Sixty-five deaths from typhus occurred the week before last in the Belgravian sub-district, 104 in Westminster, 60 in the south sub-district of St. Giles, 55 in Hoxton New Town, 96 in Mile-end Old Town, 176 in Bow and Poplar, 77 in the Borough-road sub-district, 82 in Deptford, 77 in Greenwich, 113 in Woolwich, and 73 in Plumstead. No doubt much must be laid to the circumstances alluded to by Dr. Farr as causative of the increase in cases of this fever. Still, week after week of oozy, slimy, decomposing mud in every thoroughfare of a densely inhabited town cannot fail, even at the low temperature of winter, of being most insalubrious. We are hence glad to find that the Registrar is beginning to draw public attention to this question of the accumulation of mud in our streets, an evil particularly prominent in such roadways as are macadamized. Dr. Farre writes:—

“It would be a humble but invaluable sanitary work if the streets were subjected to a perfect system of purification; if they were frequently and thoroughly cleansed and dried. The attention of Boards of Works may be well directed to this end. The present expedient, by which householders who pay rates for parochial management are expected to clean the pavement in front of their own houses, is unjust in principle and futile in practice. The whole width of thoroughfare from frontage to frontage should get the benefit of the same brooms and shovels. Old persons, whose avocations compel them to be much out of doors, in feebly urging their steps over filthy streets waste much of that strength which should be husbanded for the employments by which they live. And how can poor people be expected to keep the inside of their houses free from damp and dirt when all the adjacent exterior is a ‘mud-ocean’?”

A good scavenger is a practical teacher of that cleanliness which is said to be next to godliness; and if the streets were well kept, the crowds who frequent them would read excellent plain sermons in stones."

It would be well "if the numerous paupers and destitute poor were set to work to clear this mud away, and to make our streets dry and passable. It would be a source of honest remuneration for them, and of great comfort to the inhabitants of London." In such a vast place as London, with such traffic and such a climate, probably we must always be doomed to some amount of mud sometimes, but it should be reduced to its minimum. Our Prime Minister is said to have remarked that, after all, dirt was only a right thing in a wrong place. It may be so. All we are anxious to prove at present is, that the public streets are not the localities to be permitted for its lengthened sojourn. Mud and dirt, and all sorts of abominations, will constantly be generated; but then let us away with them as soon as possible. We kept cesspools of human excrement, &c., under our noses once, and planted them close to our kitchen windows. We have long since reformed this matter, and made clean our underground quarters. It is scarcely consistent that we should surround ourselves with a slough of despond in our higher regions. *Non-accumulation* is the great thing with all noxious and disagreeable débris. If it will form it must; but, as with our house sewage, away with it. And it is here that the blame as well as the mischief lies. Because it goes on snowing and thawing alternately, and forming fresh slush, what is the use of cleaning up at this moment? Wait a day longer, and then it may be over, say our scavenger inspectors. We might as well have waited until we did not need water-closets before we emptied our cesspools. Good street scavengering cannot be performed without cleansing and sweeping night and morning—at any rate in the metropolis. We have a strong feeling that some of our thoroughfares now macadamized, and in which the traffic has become greatly increased, should be paved. These streets are being either constantly ground to dust, as in summer, or converted into black gas-saturated mud in the winter, and are at all times troublesome to keep in order. Where the traffic is slight, macadamization is admirable; but where it is extensive and unceasing, it will not stand the wear and remain in a state passable for pedestrians. Dr. Farr suggests that there is perhaps more connexion between the London mud and the London fogs than is generally supposed. They are certainly both black and dense enough, thanks to the gas escaping below the ground, and to the "smuts" revelling above it.

"If coal were cheap, the greater command which the poor would have over that commodity would materially help to reduce the winter rate of mortality; and if smoke were abated at domestic fires, as well as at bakers' ovens and public furnaces, by more thorough combustion of fuel, the carbonaceous particles which they emit would not darken the air and pollute whatever they touch, nor by forcing a passage into the throat and lungs, aggravate or excite fatal pulmonary complaints in human beings."

ART. 10.—*On the Physiological Aspect of the Sewage Question.*

By Professor HUGHES BENNETT.

(*Med. Times and Gazette*, Oct. 1865.)

At the last meeting of the British Association for the Advancement of Science, Dr. Hughes Bennett advanced the following propositions on this subject :—

1. *That atmospheric air, strongly impregnated with odours of various kinds, is not necessarily injurious to health.*
2. *That atmospheric air without smell is often most dangerous.*
3. *That smells, as smells, are neither injurious to health, nor are they a nuisance to those who live amongst them.*
4. *That deleterious gases arising from effluvia are only injurious by being infused and carried into the blood, and to this end they must be sufficiently concentrated, and the atmospheric air proportionately diminished.*
5. *That emanations from drains and from sewage entering running streams are in no way dangerous.*
6. *That typhoid fever cannot be proved to originate with the fermentation of drainage water.*
7. *That improving drainage by costly works does not necessarily remove disease.*

On the first of these propositions Professor Bennett said he had recently come from a part of the shores of the Mediterranean where large tracts of land were employed in that country for odorous flowers. The inhabitants were, for the most part, unconscious of the odours with which the air was impregnated, but strangers were conscious of it directly. This atmosphere, however, was not productive of epidemics, and they never would have any gigantic propositions for getting rid of these perfumed plains. Then there was in one part of Paris an establishment for the distribution of manure; the smell was no doubt very great, but the advantage arising from it was so important that the establishment still existed. No great injury was shown to result from it. The Thames in 1858 was in a terrible condition. It had been distinctly proved, on the best evidence—the evidence of those who have best interest in finding out that this state of things was a source of disease—that the condition of the Thames at the time had never been productive of the slightest bad effect. When he made that statement in the Chemical Section the other day he was flatly contradicted; but he had the evidence of Professor Rawlinson and others to show that no disease at all was engendered in London from that source. He had just come from Naples, and he had paid particular attention to the condition of that city. It was a very volcanic region, and bubbles of sulphur and hydrogen were continually springing up in the sea all round the coast, while the drainage was so managed that it came all down to the bay, which was overlooked by the best houses. The drains

formed through the city were large slits, which threw the smell up to the atmosphere, and the odour was so great that he never smelt anything like it elsewhere. The people were so satisfied that this smell was injurious that rents at the top of the houses were twice as much as the rent below. He made inquiries, and found that Naples was not more subject to typhoid or other fevers than any other city, and he was convinced that there was no excess there. He had visited all the large hospitals in Naples, and amongst them the Military Hospital, which contained at the time 800 patients. The terrible dirty condition and effluvia at that hospital were such as he could not attempt to describe, and yet the medical superintendent told him that there was no fever caused in that way. There was only one case of typhoid fever in the house at the time, and there was scarcely an hospital in Europe where only one case of typhoid fever would be found out of 800 patients.

In support of the second proposition, he referred to the marshes of Essex and Lincolnshire, the low grounds of Holland, the campagna of Vienna, the delta of the Ganges, the Guinea Coast, and other parts of the world, where the most pestiferous fevers prevailed epidemically in their worst form, and yet these effects were never attributed to bad odours, nor did bad odours exist to any extent. The conclusion from these two propositions was, that there was no necessary connexion between bad smells and deleterious gases. Some deleterious gases, such as carbonic acid and others, had no smell, while there were others, such as carburetted and sulphuretted hydrogen, that had smell, but in which the absence or presence of the smell had nothing to do with their unhealthy properties.

On the third proposition he urged that people became accustomed to the smells. The sense of odour was really paralysed, as any one might test by holding to the nose for any time an odoriferous flower. They were only strangers to a neighbourhood, therefore, who detected its bad smells.

In proof of the fourth proposition, he said that workmen descending into pits and wells, where gas had been shut up, often met with danger, but those who exposed themselves to the effluvia of open drains were not affected. The men who worked in the large drains of London were not more particularly affected as regarded fever and similar diseases than others of the same class differently employed.

In respect to the fifth proposition, he stated that, according to statistics published by Dr. Littlejohn, officer of health of Edinburgh, who was employed by the magistrates of the city, and was, therefore, a witness on the other side, that the cases of fever in the year 1863 were four in the upper water district and one in the lower, out of a population of 13,321 in the former case and 3866 in the latter. Out of 17,000 inhabitants, therefore, there were only five cases of fever amongst those who lived on the border of a very foul stream; whereas, when they went to the districts surrounding the Tron Church, they found no difference in the number of cases. With regard to cholera, the cases were 30 or 40 times as many in the districts of St. Giles and the Grass Market as in the so-called

infected districts. According to the registrar-general's return, the death-rate of the Leith district was 17·62 against 24·5 over the whole city.

Regarding the sixth proposition, he argued that when an epidemic attacked a locality, it was usual to find out some obvious physical cause, and there was no difficulty in finding plenty of illustrations to support the view they adopted. His own view was that we were profoundly ignorant of the causes of epidemic disease. It had been said that typhoid fever originated with drains; and there were a great many coincidences to support that doctrine. There were a number of instances in which fevers had originated in a town where something had gone wrong, where a drain had been opened near, or where the smell was more powerful than usual. If they, therefore, confined their attention to striking coincidences, there appeared plenty of grounds for the theory. But they should inquire further. The Bradford epidemic was proved to have originated with the drinking of impure water; and the great epidemic in Edinburgh in 1847 and 1848 followed the failure of the potato crop, and ceased when the scarcity of potatoes ceased. Without going into a great many examples, he might say that there were innumerable cases of emanations that had never caused epidemics, to counterbalance those that could be cited on the other side.

On the seventh and last proposition Professor Bennett dwelt at great length. He urged:—In Paris, where great improvements had been carried out, and where the streets built by the present Emperor were arranged according to the most approved discoveries in modern drainage, epidemics had not been diminished. The old town of Edinburgh had no draining at all up to a recent period, and old pupils at the University could remember that some years ago a case of typhoid was unknown there. Typhus used to be the prevailing form, while typhoid prevailed abroad; and as foreign physicians came over to Edinburgh to study the former, Edinburgh went to the continent to study the latter. Typhoid fever, however, had recently appeared in Edinburgh, and, strange to say, it appeared to have followed the improvements there. He did not mean to say that the fever was caused by the improvements; but the coincidence was curious. It was said that the introduction of the water-closet system had brought that poison into the houses: that owing to an insufficient supply of water the noxious gases now escaped inside instead of outside. That was a very plausible argument, no doubt; but when he found that the disease in Edinburgh followed the failure of the potato crop, he was induced to believe that fevers originated from other sources than drainage. When they came to investigate what these sources were, they entered upon a very wide field of inquiry. He considered that proper food and drink had much to do with the question. He had read a great deal of the cesspools, and a paper was read at the Edinburgh meeting, in which every cesspool was said to be a focus of disease. That might be so: but not on the ground on which it was put by the author. It was quite clear that where cesspools existed the ground round them got saturated with the gas, and in that way animal matter, in a state of

putrefaction, found its way into wells. The water was thereby poisoned, and drinking such water was favourable to disease. Bad water, on the other hand, was often very bad to the taste, but it did not, therefore, follow that it should be injurious. In the case of a besieged town, when the inhabitants were left in a nervous condition, and there was a want of food, epidemics sprung up; and crowding and poverty were also fruitful sources of disease. The great seats of fever in our towns were the narrow closes, where the dwellings had been badly ventilated, and where insufficient and bad food and many other injurious agencies were at work. If he were to pick out any one of those agencies, and say that it was the cause of fever, he should be reasoning very erroneously. He had heard it said that the fastidious tastes and feelings of Englishmen were opposed to all those plans which were found to work so usefully in other countries, in France, as in Germany, and even in China. They must endeavour to prevent the public from adopting partial or erroneous views upon this question. Attention ought to be directed to the prevention of the pollution of our rivers and streams, and to the consideration of how the sewage of towns could be best utilized, so as to return to the earth the fertilizing qualities that had been taken from it. Human excrements, like those of all other animals, if properly employed, so far from being poisonous and injurious, were, in truth, the source of the growth of the vegetable, and thus of the animal world. The engineering, agricultural, and chemical parts of the question he had purposely avoided. It would be enough if they could succeed in arriving at the conclusion that emanations from human excreta, or sewage of towns, were not so dangerous or injurious to health as was generally supposed.

ART. 11.—*On the Retention of Dead Bodies in Houses.*

By Dr. BARNES.

(*British Medical Journal*, January 14, 1865.)

Dr. Barnes has drawn up certain suggestions with the view of diminishing the dangers resulting from the undue retention of the bodies of the dead in inhabited houses, and has submitted them to the General Purposes Committee of the Association of Medical Officers of Health. He says that there appear to be five principal quarters to which appeal for aid in carrying out measures for diminishing the evil may usefully be addressed:—A. The medical profession generally (through the medical press). B. The vestries, district boards, and boards of guardians (these to appeal to the public). C. The registrar-general. D. The cemetery companies and undertakers. E. The hospital authorities. He recommends as follows:—

For the Vestries. B. 1. That a circular be prepared for submission to the vestries and district boards, setting forth the dangers arising from the existing practices relating to the dead previous to burial, and containing the following recommendations, with a view

to the diminution of such dangers. The circular to be sent, in the first instance, to the medical officer of health of each board, requesting him to submit it to his board. 2. That the extreme importance of providing some suitable place for the temporary deposit of dead bodies previous to interment be urged upon the vestries and district boards. That the vestries and district boards be requested to use their influence with their board of guardians to allow the use of the parochial dead-houses for this purpose. 3. That, as a sanitary measure, the vestries and district boards issue a public notice to be permanently maintained, as by painting on boards, and by printed placards, directing attention to the public danger resulting from the undue retention of the dead amongst the living, and stating that in cases of urgency the bodies of the dead may be deposited in the parochial dead-house under the security of a responsible officer, until arrangements are completed for the burial. [The construction of regulations for the reception and care of the dead in the parochial (avoid the word "workhouse") dead-house or "mortuary" will require some consideration.] 4. That undertakers be informed that dead bodies can be received in the parochial mortuary, and be encouraged to deposit them in this place, rather than to keep them on their own premises. 5. That the undertakers and the masters of the poor-house be instructed in every case of a person dying in the poor-house of scarlatina, fever, or other infectious disease, especially to surround the corpse with charcoal. 6. That the undertakers employed by the board of guardians be instructed, when sent to bury a body from the dwellings of the poor, to surround it with charcoal. 7. That the vestries and district boards be invited to consider the propriety of applying to the legislature for the enactment, in an amended Diseases Prevention and Nuisances Removal Bill, or otherwise, of clauses empowering them, as sanitary bodies, to provide mortuaries, and their medical officers to order the removal of corpses to such mortuaries, similar clauses being contained in the City Sewers Act.

c. 1. That the registrar-general be requested to append a note to the forms of certificates of the cause of death supplied to medical men recommending the use of charcoal in the manner described. 2. That the public be earnestly invited by placard (P) or by a notice, to be given in every case by the registrar, when applied to by the friends of the deceased for a certificate of the cause of death, to see that the body is surrounded by charcoal as soon as placed in the shell or coffin.

d. 1. That a circular be sent to the cemetery companies, requesting them to establish depositories for the dead in convenient places for the reception of bodies waiting interment in their respective cemeteries. 2. That the funeral companies and leading undertakers be invited to establish convenient depositories, such as would, for decency and care, command the confidence of the public.

Measures for the Hospitals.—E. 1. That a circular be issued (either by the Association or from the vestries) to each of the metropolitan hospitals, inviting the authorities to take measures that

every dead body, before being allowed to leave their dead-houses, shall be duly surrounded with charcoal. [In support of this request, attention should be drawn to the frequent propagation of disease by the neglect of disinfection; to the fact that bodies are sometimes removed from hospitals to the house of friends or to the premises of undertakers before interment, and that the observance of this precaution would act as a great encouragement to the general adoption of similar means amongst the public.]

ART. 12.—*On the Interment of the Dead.*

By Mr. —.

(*Lancet*, November 5, 1864.)

"We boast," says our anonymous writer, "of our superior care in sanitary matters; our practice, in some things, does not justify our boasting. I refer to our customs as to the interment of the dead and the keeping of dead bodies beyond the proper time, whereby much annoyance and risk are incurred by those who may be called upon to attend at funerals in our churches and chapels. In many cases of small-pox, or of malignant fevers, no notice of the cause of death having been given by the friends of the deceased, the dead bodies of those who have died from the above causes are brought, sometimes in a putrid state, into the church or chapel where the previous service is performed in the midst of mourners and casual spectators. Our street-cabs are used as hearses, and immediately after having done this service the driver seeks for another fare.

"Burials in lead, in vaults, and in catacombs are another cause of annoyance and injury to the public. The lead coffins burst, or are perforated. In both cases they continue, for a long time, to be vomitories of stench, contagion, and disease. Dead bodies are often kept until they emit a most foul and offensive smell, and not unfrequently until decomposition has advanced so far that a fetid and disgusting liquor runs down the backs of the bearers, and afterwards in the church or chapel.

"Why should these places be thus desecrated, and the visitors to them be made to incur great risk to health and life? And all this to gratify the morbid fancy and superstitious feeling of those who think, or act as if they thought, that some benefit would accrue to the dead by these mortal remains being brought into a consecrated building. At St. John's, Westminster, and at Fulham Church, no dead body is allowed to be brought in. At Fulham Church a place is provided, under the belfry, in which to deposit the dead body, while the living proceed into the church to be present at the 'desk service;' and thus, by the wise arrangement of the late Bishop of London, much annoyance and danger are prevented.

"The practice of carrying dead bodies on the shoulders of men is most reprehensible. The poor fellows are shut up under the thick covering of a pall, fetid from long and frequent use, and made more so by its *present* use. They have, as I have sometimes seen, in

cases of pauper funerals, to carry their loathsome burden for a long distance, since the economical parochial management refuses to provide a hearse. In some such cases I have seen the bearers, after depositing their burden, roll on the grass and vomit painfully. Some of the evil of which I speak might, in part at least, be prevented by the use of the bier, which has been laid aside except in a few cases.

"By the grave-side, a few days ago, the officiating minister was heard to ask one of the *employés*, 'Which way does the wind blow?' Having ascertained this point, he said, choosing his position, 'Then I will stand here; for the person to be buried died of a bad typhus fever.' That 'person' had been carried on men's shoulders into the chapel and out again just before.

"One cause of a large part of the evils complained of is this: the business of undertaker is now often in the hands of inexperienced, needy, and reckless men, by whom decency and all sanitary considerations are neglected. Would it not be well to cause all undertakers to take out a licence and to be registered? Thus a certain amount, if not of respectability, at least of responsibility, might be secured. Some of these men will not use the commonest precautions, even a little sawdust in the coffin, or, in bad cases, pounded charcoal.

"An effectual remedy for these evils, or, at least, some of them, would be the passing of a law making it imperative to bury the dead in a decent time after death, and before offensive putrescence had taken place. What can be more disrespectful to the dead than to keep the mortal remains until they are a nuisance and an injury to the survivors? Perhaps the appointment of local sanitary officers might be useful in carrying such a law into effect. It is high time to apply some remedy to this crying evil."

ART. 13.—*On the Necessity for separate Wards for
Fever Cases in General Hospitals.*

By Dr. —.

(*Lancet*, February 11, 1865.)

The remarks which follow are taken from a critical analysis of a report by Dr. Bristowe and Mr. Holmes, "On the Hospitals of the United Kingdom," contained in the "Sixth Report of the Medical Officers of the Privy Council." The reporters think that cases of fever (typhus and scarlatina) may be admitted into the ordinary wards when these wards are capacious and well ventilated; but they do not, as it seems to us, support their opinion by satisfactory evidence. At any rate, they say nothing which, to our mind, sets aside the evidence of a contrary character which is contained in the following remarks. Our anonymous critic writes:—

"1. The nurses and attendants on patients sick of typhus must, almost as a matter of necessity, take the disease sooner or later, provided they be not already protected by a previous attack.

Science has not yet discovered any means for preventing their infection, and probably never will. Seeing, then, that such deplorable results cannot be prevented, it is surely our bounden duty, when a given number of typhus patients have to be treated, to expose as few nurses as possible to the risks of infection; and this desirable end is more likely to be attained by having one ward for typhus, than by admitting typhus into every one of the medical wards of an hospital. Although the facts have been kept comparatively quiet, yet if a list were made of all the sisters and nurses who have contracted or died of typhus in the general hospitals of London during the present epidemic, the result, we believe, would be perfectly appalling. If the typhus cases were restricted to one ward, or one set of wards, the nurses no doubt would not long escape infection; but if they passed through the attack safely they would be seasoned against any future attack. On the plan of isolation, the number of nurses whom it would be necessary to subject to the ordeal of seasoning would be much smaller than on the plan of scattering. In the latter plan also the practical advantage derived by the community from their seasoning would be small, as the number of typhus patients who would afterwards come under their care would be few in comparison to what it would be on the plan of isolation. Moreover, it is well known that nothing influences the rate of mortality of typhus to a greater extent than age. Under twenty years the mortality is less than five per cent.; after this it increases progressively, until above fifty years it rises to more than fifty per cent. Now, on the plan of isolation, it would always be possible to select young nurses for the typhus wards; but, on the plan of scattering, old and young must be equally exposed, and the services of the most experienced sisters and nurses must often be for ever lost.

"2. On the plan of scattering, typhus is apt to spread among the other patients; on the plan of isolation, this is next to impossible. It requires but a slight acquaintance with the history of typhus to know how very commonly the admission of one or two patients into a general ward has caused the disease to spread. To go no further back than the present epidemic in the metropolis, we believe that there is scarcely a single general hospital in London, into which typhus has been admitted, where the disease has not spread amongst the patients, in several cases to an alarming extent. We are sure that we are within the mark in saying that during the last three years 150 patients have contracted typhus in the general hospitals of London. It seems to us idle to assert that the spread of the disease in these cases has been due to want of proper ventilation, or to improperly close personal intercourse with the sick; and that by proper precautions it might have been prevented. This explanation has been urged by the upholders of the plan of scattering for twenty years or more; but a knowledge of the remedy has had no effect in curing the evil. Make what rules we will, it is impossible in practice to prevent improper intercourse between convalescent patients and the sick; and with regard to ventilation, although we are far from denying its power in preventing the spread of typhus, nurses, even the best of them, will shut windows when they ought to be open.

Human carelessness can never be ignored in forecasting the probabilities of the spread of fevers in a general ward; and surely valuable lives ought not to hang upon the absolute perfection of hospital nurses, when a simple remedy is in our power. That remedy is to treat the cases of infectious fever in separate wards. In proof of the efficacy of the remedy we may mention one fact which we notice in the report for 1862 of the London Fever Hospital, where the plan of isolation is followed. In the year 1862 there were admitted into the hospital 1827 cases of typhus, 182 of scarlatina, and 365 cases of diseases which were not infectious. Not one of the 365 patients contracted either typhus or scarlet fever in the hospital. With the evidence before us, we cannot but regard it as a most unjustifiable practice to admit patients suffering from some trifling disease, such as quinsy, rheumatism, or dyspepsia, into the wards of a general hospital, and to make them run the risk of catching typhus or scarlatina, and of thus losing their lives. A little reflection must lead to the conclusion that this is misdirected charity.

"In connexion with the above remarks, it may be mentioned that Dr. Murchison has compared the results of the treatment of typhus in the London Fever Hospital during the first six months of 1861 with those of six of the general hospitals of the metropolis during the same period. The six hospitals were St. Mary's, St. Bartholomew's, St. Thomas's, Guy's, Middlesex, and the German Hospital, and the results were as follows. The 1080 cases admitted into the Fever Hospital communicated the disease to 27 persons, of whom 8 died. In other words, only 1 person took the fever for every 40 admitted, and only 1 died for every 135. But the 272 cases admitted into the six general hospitals communicated the disease to 71 persons, of whom 21 died; or 1 person caught the fever for every 3·8 cases admitted, and 1 life was lost for every 12·9 cases admitted."

The reporters think that it is impossible to say what credit is due to the above statement, "since the fact that 71 cases of typhus commenced in persons who were inmates of these hospitals during the period in question might be susceptible of other explanations than that they caught fever from the patients." (p. 472.) What these explanations are we are not told; but only two can suggest themselves to us—either that the poison originated in the hospital, or that it was conveyed from outside. The latter seems hinted at in another part of the Report (p. 542), where, in accounting for the outbreaks in two of the hospitals, it is stated that both "occurred during the prevalence in London of a very severe typhus epidemic." But these explanations do not appear to us to be valid in face of the fact that typhus had not been known in any of the six hospitals for months or years before the outbreaks in question, and that these outbreaks did not occur until after the admission of cases into the hospitals; while it is surely more probable that the poison was derived from patients in the hospital than from persons outside.

"3. Provided there be ample space and good ventilation, the rate of mortality from typhus is not greater in a fever hospital or in fever wards than in the ordinary medical wards of a general hospital. This appears to us to be satisfactorily shown by Dr. Murchison's

statistics. During the six months above mentioned, the rate of mortality from typhus in the Fever Hospital was 20·95 per cent., and in the six general hospitals 23·32; a result the more extraordinary considering the aged and infirm class of patients admitted into the Fever Hospital. It is to be regretted that Dr. Murchison does not draw a distinction between the mortality of patients admitted with typhus and that of persons seized with typhus in the hospital. It must be remembered that the rate of mortality in the general hospitals is increased by the fact that many of the patients who contract typhus in the hospital are already suffering from serious organic diseases, which add greatly to the danger of the typhus attack; but this very circumstance is itself a most powerful argument against exposing such persons to the risk of typhus.

"4. The efficacy of thorough ventilation in impeding the spread of typhus is undoubted, but experience teaches that no hospital has yet been constructed in which typhus has not occasionally spread after its introduction into the general wards. A very powerful objection also to the practice of scattering typhus patients through general wards lies in the fact that the amount of ventilation which is necessary for the successful treatment of the typhus patients would be positively fatal to many of the patients treated in ordinary medical wards; for example, to patients suffering from acute nephritis, rheumatism, bronchitis, and pneumonia. The admission of typhus patients into a general ward exposes the other patients to imminent danger, not only of contracting a most mortal disease, but of having their original maladies aggravated by the necessary ventilation. Mr. Simon appears to have felt this difficulty when he wrote as follows:—

" 'But meanwhile, (without pretending to dogmatize on a subject which I practically know to involve a great variety of considerations,) I would express a doubt whether, with our present appliances, the real minimum of the difficulty can be attained unless, in the warding of patients, cases be classified, as far as possible, on the basis of their respective requirements of ventilation and powers of tolerating cool air.' (p. 68.)

"5. With regard to scarlet fever our experience differs from that of the reporters. We have known many instances in which it has spread in the wards of general hospitals. The whole history of the disease also shows that ventilation has little or no power in preventing its spread. The poison of scarlatina propagates itself by fomites rather than through the atmosphere, and the disease ought, in our opinion, to be excluded from general wards as rigidly as small-pox. Space, however, will not permit us to enlarge further on this point.

"In concluding our remarks on this head we have only to repeat that, while fully agreeing with the reporters that every hospital which excludes infectious diseases fails in one of its most important duties, we are fully convinced that the plan of interspersing such patients through the ordinary wards is a crime against humanity which cannot be permitted long to continue."

ART. 14.—*On the Hygiène of Hospitals.*

By MM. —.

(Archives Générales de Médecine, Janvier, 1865.)

The following report on the hygiène of hospitals was adopted unanimously at a meeting of the Chirurgical Society of Paris, held on December the 14th, 1865. The object of the society, in drawing up the report, was to suggest to Government the adoption of its conclusions in the rebuilding of the Hôtel Dieu.

1. A hospital should be situated in an open place, on a dry soil, and on a declivity. The grounds should be extensive. A superficies of 50 square metres, to each patient, is the minimum that should be allowed; and more should be given if possible. This amount must, besides, be *progressively* increased in proportion to the number of patients.

2. The atmosphere of a hospital will be purer in proportion to its distance from densely-crowded quarters. In the centre of towns, there should only be retained hospitals for cases of emergency and hospitals intended for teaching. Healthiness and economy would thus be studied; and large towns, like Paris, would be able to build hospitals on extensive grounds, purchased at a small cost.

3. Good hygienic conditions are easily obtained in hospitals of from 200 to 250 patients. They become nearly impossible in large towns, if more than double this number of patients be brought together. Within those limits, expenses of all kinds are not greater than in the case of more crowded hospitals.

4. As the constituents of the atmosphere mix chiefly in a horizontal direction, the effects of contact and proximity, which constitute overcrowding, and which take place from patient to patient, from ward to ward, from building to building, should be combated by allowing spacious accommodation.

5. It is not only by increasing the cubic space allotted to each patient, but also and chiefly by increasing the superficial area, which is at present insufficient in our civil hospitals, that contagious influences will be efficaciously combated. For similar reasons there should be no increase in the number of stories, each of these producing a more or less vitiated atmospheric stratum. In a rigorous hygienic point of view, more than two rows of patients should never be placed one above the other.

6. It would be illusive to believe that a good allowance of air inside the wards can replace want of space and aëration outside, or to believe that abundant artificial ventilation can supply the absence of one or other of the preceding conditions. Nothing can remedy the insufficiency or the want of natural aëration.

7. Buildings, completely isolated, looking in the same direction, exposed directly to sunlight, to the influence of rain and winds, should be built in one row, or in parallel rows, at wide intervals of from 80 to 100 metres, so as to obtain an efficacious separation, and a free and easy aëration outside.

8. Small wards, containing from 15 to 20 beds, are easily kept under supervision as regards treatment; the patients are less in one another's way; the risks of contagion are less; all impurities are more rapidly taken away. They should be preferred for ordinary cases, without interfering with the special dispositions which are needed for certain classes of disease requiring greater space and *isolation* in separate rooms.

9. The furniture in the wards should offer no obstacle to the circulation of air. It is necessary that the medical attendants should have the right of having the bed-curtains removed, when they think proper.

10. The wards should be separated by the landings and domestic offices. It would be advisable to have one room, in which those patients who can get about might have their meals and sit during the day. The ward would be thus daily, however incompletely cleared.

11. The periodical and regular clearance of wards, and their being left unused for several months, give, in French military hospitals and in foreign hospitals, results which show that a general adoption of this plan is particularly imperative in epidemic seasons.

12. Arrangements should be made in order to rapidly destroy or remove all smelling matters, excreta, dressings, water which has been used, &c. They should never be kept inside or near wards occupied by patients, and should not be allowed to give off any appreciable exhalations.

13. It is recommended that, besides the central administration of hospitals, a permanent Board of Hygiène and Health be appointed, holding periodical meetings; and that the board consist of physicians, surgeons, managers, engineers and architects, and be empowered, according to circumstances, to add to their number, with the privilege of voting, all the hospital physicians and surgeons who are not already members of the board. Periodical meetings of the physicians, surgeons, and managers attached to each hospital would give to the administration of hospitals information, which would enable it more safely to proceed with the improvements already attempted.

This last measure, which is in conformity with wishes expressed at the Academy of Medicine, would only be returning to old and useful customs.

ART. 15.—*On the Physical Effects of Work in Compressed-Air Chambers or Caissons.*

By Dr. HERMEL.

(*Annales d'Hygiène Publique et de Médecine Légale*, January, 1865.)

This question has recently assumed some importance in connexion with engineering operations. M. Hermel has subjected it to a careful examination. In caissons, whether the atmosphere be compressed or not, the air is confined—that is to say, it is not renewed,

except by leakage, which is carefully avoided as much as possible, and by the infrequent opening of the caisson at the time of entrance or exit of the workmen. Now, in the air thus confined, many men breathe, and a certain number of candles are burned, from whence absorption of the oxygen, and the formation (1), of *carbonic acid* gas, eminently obnoxious to respiration, and of which the exhalation is augmented by muscular exertion; (2), *carbonic oxide*, a deleterious gas; and (3), smoke produced by the imperfect combustion of the candles. Thus, the individuals working in the caissons suffer from an incipient asphyxia analogous to that determined by charcoal vapour. M. Hermel finds the proof of this in the ruddiness of the blood as it flows from a vein, a fact frequently noted in the observations of MM. Pol and Datelle, and which they attributed to a sur-oxygenation of the blood. But M. Hermel observes, in accordance with M. Francois, that the compressed air does not contain a larger quantity of oxygen. The air is compressed, but no change takes place in the relative proportion of its elements. The oxygen of the caissons being considerably diminished by respiratory absorption and combustion, and the carbonic acid disengaged interfering with the arterialization of the venous blood, how does it come to pass that the blood is sur-oxygenated? Whatever the reply to this question may be, it is well known, that, according to the experiment of M. Marge, the blood drawn from the veins of a person asphyxiated by charcoal vapour, presents the vermilion colour of arterial blood.

Thus, the less frequency of respiration, the diminution or acceleration of the circulation, according to circumstances, the enfeeblement of the sight, the augmentation of transpiration, &c., which are experienced by workers in the caissons, denote a state of semi-asphyxia. To this cause, conjoined with mineral emanations in muddy subterranean and sub-marine works, is to be assigned the progressive wasting which, after a certain time, affects the workmen, and gives them the aspect of persons recovering from grave sickness. But this is not all. A state of extreme *malaise*, and a sensation of suffocation, has been noted during the passage from the cylinders to the chamber of supply. In the narrow shafts, the atmosphere is worse than in the chambers, in consequence of the greater accumulation of deleterious products rising from below, and the smaller space. In the supply-chamber, the relative rarefaction of the air produces effects analogous to, if not identical with, the so-called mountain-malady (*mal des montagnes*). Moreover, the medium there is still vitiated by the disengagement of abundant vapours, which provoke cough, and produce a mistiness of the eyes sufficient to prevent vision. This noxious and rarefied atmosphere acts upon men already fatigued and half-asphyxiated; and the pulse is found to mount even to 130, the respiration becomes accelerated, perspiration breaks out, and the warmth of the body is rapidly lost.

The various evils noted by authors as arising from the conditions described will be due to the serious interference with the arterialization of the blood by the vitiation of the air in the caissons; and M. Hermel asserts that the gravity of the evils is in proportion to the degree of compression, the length of sojourn in the compressed

atmosphere, and the rapidity of decompression. When the condensation of the air does not exceed a pressure of $1\frac{1}{2}$ to 2 atmospheres, morbid affections, more or less slight, are observed—*e. g.*, otitis, otalgia, bronchitis, and muscular pains. When the condensation exceeds two atmospheres, more serious evils occur—*e. g.*, persistent disturbance of vision, abscess, emphysema, and stammering; pulmonary and cerebral congestion, followed by paralysis of the viscera and members, disturbance of memory and ideation, and sometimes sudden death.

As prophylaxis, M. Hermel recommends that, for a three hours' turn of duty, 1620 litres of oxygen should be supplied for each workman; for a turn of four hours, 2160 litres. The carbonic acid should be absorbed by lime-water. A regular flow of air should be maintained by means of valves, providing for the constant renewal of the atmosphere. The transition from the compressed to the uncompressed air should be slow, and the transition period, as M. Bugavy observes, should count as part of the turn of duty. The size of the supply-chambers should be increased; and M. Hermel suggests that there should be two chambers, in one of which the air would be compressed to one-half, the other to one-fourth, the degree to which the atmosphere in the caissons is subjected. In the first chamber the man would be delayed a definite time. In this manner the entrance and exit from the caissons would be gradual.

It is important that no person should be permitted to enter the caissons who is in drink, or who suffers from any organic affection of the lungs or heart.

M. Hermel finally directs attention to two instances in which caissons, from a faulty construction, have exploded with fatal consequences.

ART. 16.—*On the Special Hygiène and Pathology of Workers in Copper in Naval Arsenals.*

By Dr. C. MAISONNEUVE.

(*Archives de Médecine Navale*, Janvier, 1865.)

The general conclusions of this memoir are the following:—

Metallic copper, when worked cold, produces no injurious effects. These are only observed when the fused metal is poured into moulds, or in workshops where molecules of oxide and carbonate of copper float largely in the air, and may get introduced into the pharynx and air-passages.

Very intense dyspnoea, with laryngeal and bronchial spasm, follows upon the introduction of a large quantity of particles of copper into the air-passages. Colic is also brought on through irritation of the mucous membrane of the alimentary canal by the molecules of copper, which, during inspiration, get into the mouth and pharynx, and when afterwards swallowed pass into the stomach and intestines. The colic may be accompanied by vomiting or by diarrhoea; it does not last long, and the attacks are not serious in the immense

majority of cases. The presence of colic does not indicate previous chronic poisoning of the system at large.

Workers in copper are not, from their occupation, prevented from having a good constitution and excellent health; but the sickly look of the men employed in naval arsenals is due to the fatigue resulting from incessant hard work, to poverty, and often also to various excesses which they commit.

Sixty-eight workmen were examined by Dr. Maisonneuve, and two-thirds at least of the number stated that they had, at one time or other, suffered from pain seated at the upper and middle abdominal region. This painful sensation, which they termed *colic*, was increased by pressure; in some cases it was limited to the pit of the stomach, in others it was seated a little lower down, between the epigastrium and umbilicus, somewhat about the position of the transverse colon. In all cases there is nausea, in some vomiting, in a very few diarrhoea. There is occasionally some fever. The attack is generally of short duration, and in most cases it is over by the next morning, rarely extending over two or three days. The workmen do not apply to a medical man, and treat themselves by drinking large quantities of milk. They are not all affected in the same degree, and some escape entirely.

ART. 17.—*On the Manufacture of Aniline Dyes.*

By Dr. BERGERON.

(*Gaz. Hebdomadaire de Méd. et de Chir.*, Février 3, 1865.)

The manufacture of aniline dyes comprises a series of complex processes, during which very different substances are produced or used, some of which are innocuous, whilst others exert a more or less injurious influence on the health of the workmen.

Thus, the vapours of benzine, which are, besides, slightly concentrated in aniline manufactories, and the vapours of acetic acid, seem to exert no action whatever. The ruddy vapours of hyponitrous acid, on the contrary, sometimes give rise, as is well known, to symptoms of poisoning especially affecting the respiratory passages. As to the exhalations of nitro-benzine and aniline, they give rise to very variable functional disorders. Thus, there are frequent but slight and transient symptoms of gastric disturbance; the nervous system is affected, as shown by the presence of headache and giddiness, which, in general, disappear after a few weeks. Syncope also occurs, and in some exceptional instances much graver phenomena are observed, such as coma, complicated with delirium and convulsive movements. Experiments on animals have reproduced and exaggerated some of the accidents observed in the workmen; and they have shown that, whilst nitro-benzine stupifies, aniline, on the contrary, excites the muscular system energetically. These two substances can again produce some analgesia of the upper limbs, and, in exceptional cases, localized muscular paralysis. Experiments made on animals, however, under circumstances as nearly as

possible analogous to those under which the men work, have never been followed by that phenomenon. Aniline and nitro-benzine do not appear to act specially on the genital functions, which, in some instances, merely participate in that languid condition of the whole organism, which is, after a lengthened period, brought on by carburetted vapours. A constant effect of these vapours is to give rise to an anæmic aspect in all the workmen, apparently out of proportion with the exertions they have to make. This striking contrast would alone suffice to show that there is no real chloro-anæmia, if the absence of palpitations and of cardiac or arterial bruit, and especially the rapidity with which the natural tint of the skin reappears, did not concur in proving that, in these cases, the alteration of the blood is not very grave, and cannot certainly be characterized anatomically by a diminution in the number of red corpuscles. There is merely, in all probability, a discoloration of blood corpuscles, either from a direct action of the carburetted vapours constantly brought in contact with that liquid through the respiratory passages, or indirectly from a diminution of oxygen in the air inspired. There may perhaps be displacement of oxygen, and substitution of these carburetted gases, and sequential modification in the shape of the globules. Under the microscope, besides, the blood-cells look depressed, and show no tendency to pile themselves (the latter phenomenon being particularly marked in animals). Later, real chloro-anæmia may supervene, with all its accessory and characteristic symptoms. The only bad results which, in the manufacture of aniline dyes, can be ascribed to arsenic, which is used largely in the process, are vesiculo-pustular eruptions, and those ulcers which have been so often noted in all the cases when arsenical compounds are used.

ART. 18.—*On the Prevention of Syphilis among the Civil Population.*

By Mr. —.

(*Lancet*, March 4, 1865.)

At a recent meeting of the Epidemiological Society an important discussion took place upon the prevention of syphilis amongst the civil population. The discussion was well-timed. Efforts are being successfully made, under the provisions of the Contagious Diseases Prevention Act, to limit the prevalence of venereal maladies in the army and navy. A Government Committee is now also sitting to investigate the nature and treatment of syphilis, with special reference to the public services. It is fitting, therefore, at a time when the Legislature has become awakened to the great moment of this question, that the interests of the civic population should not be overlooked. Amongst them syphilis, although less patent, is not less injurious than amongst our soldiers and sailors. Few maladies excite more the apprehension of medical philanthropists; still fewer conduce so greatly to the physical deterioration of a people.

The subject was brought before the Epidemiological Society by Dr. Babington. He first touched briefly upon the objections commonly urged against legislative interference with prostitution—to wit, the arbitrary meddling with the liberty of the subject, and the moral sanctioning by implication of incontinence. To both objections Dr. Babington demurred. He held that by “freedom” is meant the power or right to do everything we choose which is not morally wrong, or inconsistent with an equal right in others to do the same. But freedom to do what is right within these limits is inconsistent with freedom to go beyond them. Thus, to do what we like with our own property is inconsistent with doing what we like with the property of others. If a thief were as free to steal another’s property without punishment as an honest man is free to do what he likes with his own property, there would be an end to all law and order in society. It seems, therefore, Dr. Babington cogently argued, that the freer the country, or, in other words, the freer every individual is to do as he likes within the bounds of rectitude, the more stringent should be the laws of restraint upon those who desire to do wrong; and hence that we, who are a free people, have more right than our neighbours across the Channel to make laws restrictive of vice.

To control prostitution is not thereby to sanction it. There is a wide distinction between approval or sanction and toleration. We tolerate gambling to the extent of allowing men to bet upon horse-racing and other events; yet we cannot be said to approve of it, for we place it under restraint by prohibiting lotteries, betting-houses, and gambling-booths at racecourses. We do not approve of drunkenness; yet we neither interdict the use of spirits, nor interfere with the drunkard himself unless he publicly exposes his vice. In that case, however, we fine him; and, moreover, we put a high duty on spirits, register and visit distilleries, and punish illicit distillation. The evils of drunkenness are very great; yet not so great as those which arise from the infection of syphilis. Were the latter confined to the individual, they might be regarded as a just punishment for the commission of sin, and be of consequence only to its perpetrators. But it is never to be forgotten that syphilis is hereditary; that the offspring of those parents, whether father or mother, who are tainted with it, may suffer for generations to come. The innocent expiate the sins of the guilty; the nation itself is deteriorated; and hence it becomes a legitimate question whose province it is to provide for the public good. Dr. Babington could not see why the Legislature should not, for the sake of establishing greater freedom amongst the virtuous, interfere with the freedom of the vicious. As Staff-Surgeon Dr. Crawford aptly and ingeniously put the question during the discussion, prostitution is already legalized; we in reality protect the woman in propagating a loathsome disease.

Dr. Babington thought that the obstacles to carrying out a system of registration and examination of prostitutes were not insurmountable. As Mr. Radcliffe subsequently showed, the practical difficulties in the way of such a system were much exaggerated. Public sentiment, as regards interference with the “liberty of the subject,”

was, so far as prostitutes were concerned, very much astray. The Legislature looks upon prostitutes as members of the criminal classes, and amongst these classes all prostitutes who are known to the police as living in brothels, walking the streets, or otherwise openly following their avocation, are enumerated in the Judicial Statistics prepared annually for the Home Office, and laid before Parliament. The prostitutes known to the police are those who require to be dealt with by the Legislature. Now the number of these returned for England and Wales in 1863 was 28,800; the number of brothels 7204. It is to be remembered that there is no question here of the entire number of prostitutes in the kingdom. It is simply the number known to the police which concerns us. The statistics referred to are the only trustworthy figures we possess on the subject; they have been collected on a definite plan for several years; and it must be obvious that any suggestions for a police regulation of prostitution must be based upon them. It is gratifying to find that the number of prostitutes who would at first need to be placed under sanitary supervision is so small; and that as a consequence neither the difficulty of supervision nor its expense would be so great as is commonly believed. Indeed, the Judicial Statistics bring the question within very narrow and practicable limits.

Of the excellent results to be derived from systematic sanitary regulation of prostitutes, Dr. Stuart, Dr. Smart, R.N., and Dr. Dickson, R.N., in the course of the discussion, cited several striking illustrations from the experience of certain regiments and ships in India and China. Dr. Smart, moreover, mentioned that both at Portsmouth and Chatham much good had already arisen from the Contagious Diseases Act. The results of the Act, according to Drs. Hardie (73rd Regt.) and Cunningham (60th Rifles), had been less fortunate at Shorncliffe and Aldershot, owing to the neighbouring civil hospitals refusing to co-operate in carrying out its provisions. Dr. Robinson (Scots Fusilier Guards) remarked that a plan of a similar character to one which he had long ago submitted to the authorities in reference to the Guards would probably have to be carried out before the full benefit of the Contagious Diseases Act could be reaped—namely, the establishment of Lock hospitals in garrison towns, the expense to be borne partly by the Government and partly by the local authorities. Dr. Dickson also commented on the imperfections of the Act, in making no provision for the registration of prostitutes, and the issue of health passports in their migrations from town to town. Another defect of the Act mentioned by Dr. Septimus Gibbon was the absence of any provision for payment of the medical officers who carried out its requirements.

A bill providing for the registration and inspection of prostitutes, Dr. Babington thinks, would probably cause as little sensation, and meet with as little opposition, as was encountered by the Compulsory Vaccination Act. It is tolerably certain that few, if any, impediments would be offered by the prostitutes themselves. They would be gainers in so far as registration and inspection would necessarily involve support and treatment during sickness if syphilis were pre-

sent. To meet the latter needs, Lock hospitals would be requisite in all the great centres of prostitution.

But, assuming that we are not prepared to introduce a sanitary supervision of prostitutes, are there no means of improving the present state of affairs? Dr. Babington answers in the affirmative. He suggests that, in this event, some special public provision should be made for the medical treatment of syphilis, such as would induce diseased prostitutes to place themselves early under treatment. The system pursued at general hospitals and public dispensaries, he believes, repels them. At least, it is too commonly the case, as Mr. de Méric forcibly showed, that the woman only presents herself for treatment, under existing circumstances, when she is unable from suffering any longer to ply her revolting trade, and when she has spread the disease in many directions. Dr. Robinson also spoke of the horrible condition of some of the women haunting the vicinity of the barracks in Hyde Park, causing infinite mischief amongst the soldiers, and for whom some special provision would be a boon, while to the public it would be a great economy and benefit.

Dr. Babington suggests the establishment of houses of recovery destined for infected prostitutes; and the appointment of district medical officers—who would be paid out of the rates, somewhat in the same fashion as the public vaccinators—to whom the infected could resort. He further suggests that these special officers should be placed under the control or guidance of the Medical Department of the Privy Council, or of a medical board, on the principle of the late Vaccine Board, expressly formed for the purpose.

In addition, he suggests the formation of a great philanthropic society for the express purpose of combating the monstrous and crying evil of syphilis, and framed after the manner of the Society for the Suppression of Vice, the Royal Humane Society, or the Royal Fire-Escape Society. This society he would designate—so as not to shock the delicacy of the most refined—the “Health Union.” “We are raising millions,” he observed, “to provide for the spiritual wants of a people. Is it not next in importance to this to get rid of a disease which counts its victims by myriads, and descends from generation to generation?”

ART. 19.—*On an Improved Method of Unloading and Purifying the Holds of Contaminated Ships.*

By Dr. LEROY DE MÉRICOURT.

(*Archives Générales de Médecine*, Février, 1865.)

The chief improvements, proposed by the author of the memoir, on the method at present in use, are the following:—

The men employed for unloading should, before going down the lower portions of the ship, be provided with Bougayrol's respiratory apparatus, which is very simple, and based on the use of compressed air.

In order to completely purify ships that have been gravely contaminated, the author proposes to substitute for the method at

present in use, the measure suggested by M. Lapparent, and which consists in charring the surface of the interior of ships by means of jets of burning gas.

(B) CONCERNING ACUTE DISEASES.

ART. 20.—*On the Use of Wine in Fever.*

By Dr. HENRY KENNEDY, Physician to the Cork-street Hospital, Dublin.

(*Lancet*, January 7, 1865.)

"The use of wine in fever," says Dr. Kennedy, "is a very complicated problem. In Dublin we do not use this agent because we have fever to treat, but because the case requires it. The following are some, and only some, of the points on which the judgment can be formed:—The age of the patient, as, for instance, whether he be twenty or fifty; the constitution, as to being fat or thin; the habits, as to sobriety; the position in life, as to being in the middle or lower classes; the period of admission to hospital, as regards the duration of the attack; whether the patient have received any and what preparatory treatment; the state of the frame, as to animal heat, lividity, &c.; the state of the circulation and sounds of the heart (the latter as laid down by Stokes); the early or late appearance of the spots, and the several points connected with them; the presence or absence of any complication; the character of the reigning epidemic; the question whether wine be needed at all, or whether it will agree; &c., &c.

"Now one and all of these points are most important in guiding our judgment on the matter; and when I see 500, or even 100 cases of spotted fever tabulated in conformity with these several headings, I may then give more weight to tables formed to regulate our practice in the giving of wine. For myself, I believe it to be literally impossible; but should any attempt the herculean task, I would suggest their excluding all cases except where wine, in greater or less quantity, was given. For it will be recollected that the object of the writer was to give a less quantity of wine in fever than in common use—not to exclude it entirely; and why a number of cases in which no wine at all was used have been put into the table I cannot conceive. My memory now goes over a period of twenty-five years, and during all that time I have never seen cases of typhus fever treated otherwise than by giving wine to some and none to others. The late Dr. John Crampton—than whom there was no greater master of fever—never acted on other principles; and Dr. Wade, of Birmingham, did good service when he stated lately, in the pages of the *Lancet*, that when a pupil at the Meath Hospital, Dublin, he had seen Dr. Stokes treat many cases of typhus fever without any wine whatever. So the fact is established, that in Dublin, and for a long period, some cases of spotted fever have been treated successfully without wine. And this reminds me of a fact well known to us here, that in certain years the disease requires

much more wine than in others, even to double the ordinary quantity; and this point certainly does not tend to simplify the question under discussion."

ART. 21.—*On the Treatment of Fever.*

By Dr. WILKS, Assistant Physician to Guy's Hospital.

(*Lancet*, January 28, 1865.)

"With respect to the treatment of fever," observes Dr. Wilks, "I adhere to the old practice of the hospital—that which was laid down by Drs. Bright, Addison, and Barlow, in their lectures, and which they adopted for so many years with success. The teaching of these professors was to the effect that a large number of cases ran their course without any other treatment than careful watching and feeding; that they required no other medicine than a simple saline; but that some needed a stimulant during the progress of the disease, and others required it from the commencement. The question, therefore, with regard to the administration of stimulus, was *when* to give it, and in what quantity. At the present time there are advocates for a universal method in favour of alcohol in all cases of fever, just as there are those who indiscriminately administer ammonia in scarlet fever, and who, when failing to prove its value in all cases, fall back upon the explanation that if alcohol or ammonia be of real service in a bad case of fever or scarlatina, and if these remedies do no harm in the milder forms, it is a good rule to administer them universally. Such a method is not only unscientific, but I believe positively injurious; for in many cases of typhus fever in young people, where the brain has been involved, I have a very strong opinion that the brandy which I have seen given in such cases has been positively hurtful. In my intercourse with medical men, I judge that very many are scarcely alive to the fact that typhus fever is very rarely fatal in young persons—the prognosis, indeed, only becoming serious as years are added to the age of the patient,—and, therefore, that they are too apt to attribute recovery to their remedies. Young persons always do well if left alone. Of this fact I could now quote a large number of cases in proof; and, on the contrary, the few instances which I have seen end fatally have been those in which a large amount of stimulus was given from the commencement of the disease; and, what perhaps is even more to the point, the withdrawal of stimulus in some cases where it was adopted as the method of treatment, has been attended with the most decided advantage.

"Although the subject of fever and its treatment may appear exhausted, yet this difference of opinion respecting the use of alcohol shows that this therapeutic struggle must continue for some time longer: for whilst we are witnessing the free use of stimulants in fever, we are reading that it was Dr. Graves's plan to administer antimony in the same disease. When deliberating on the merits of such contradictory treatment, many escape the

dilemma by believing that different diseases have been the subject of treatment, and that fever has changed its type; they assume, without even a sceptical thought passing through their minds, that the favourable issue was due to the remedy, and therefore their conclusion is a logical one. Such persons, however, have no right to frame this or any other opinion as to treatment unless they are conversant with the natural history of the disease; for, did they quite realize to themselves the fact that in young persons typhus fever is rarely fatal, they might with great advantage state the argument in another form; as, for instance, Will a few doses of antimony, or a few daily ounces of wine, or the abstraction of a few ounces of blood, be sufficient to kill a patient suffering from a disease the tendency of which is to subside spontaneously in the course of a few days? Surely, too, every medical man must have seen cases, either under his own charge or his neighbour's, where, from a mistaken diagnosis of the nature of the case, a remedy has been given which, above all others, he would have discountenanced had his opinion been otherwise. He must, for instance, have seen several grains of opium administered daily on the supposition that the case was one of delirium tremens instead of fever, and yet the patient has done well. If he saw many such instances, he might consider that he had good reason to believe in another change of type. Without, therefore, denying that fever may annually change in character as to some minor features, yet I believe that the argument of its great alteration, founded upon the administration of remedies, to be a most fallacious one.

"I would not wish to dictate rules of treatment to any of my medical brethren; but my own opinion is that expressed in the first few sentences of these observations. I believe that support and a moderate amount of wine is the best treatment; but I assert that alcohol is not an antagonist to the fever, which runs its course in spite of the administration of the stimulant. I would not say that in many cases of typhus in young people a little wine may not be useful; but the fact still remains, which cannot be gainsaid, that such cases would do well without any stimulant whatever. When the ground is thus cleared we shall know better what we are doing. I might mention that the only two cases which I have seen fatal of late have been those of two students, to whom a large amount of stimulant was given, and who had the care of the most assiduous nurses both night and day. In one of these there were constant convulsive movements during the last five days of life, with coma vigil, and other symptoms resembling those in which the late Dr. Graves would have administered antimony. In this case there was no albumen in the urine, nor in another, which recovered, where convulsions were most violent. Albumen has, however, been very frequently present in other instances."

ART. 22.—*On the Phenomena of the Crisis in Typhus Fever.*

By Dr. GAIRDNER, Professor of the Practice of Physic in the University of Glasgow.

(*Lancet*, Jan. 21, 1865.)

In a Clinical Lecture, delivered at the Royal Infirmary at Glasgow, Dr. Gairdner makes the following remarks upon this subject :—

"As a great number of patients affected with typhus fever get well, while a certain proportion die, I need hardly point out the immense importance of noting the circumstances which determine one or other of these results, and particularly the symptoms which, in particular cases, give you the earliest assurance that the disease is going to abate." To this inquiry we have given particular attention, and the result is, in the first place, that the well-known term *crisis*, in the old and figurative but still strictly true application of it to some febrile diseases, is scarcely a description of what occurs in the fever with which we have had to do. Let me recall to your remembrance the meaning of that word. Etymologically it presents to the mind a mysterious supernatural authority or law—a Nemesis (let us say) passing *judgment* on the issue of the disease, with power to make or mar, and that instantly. At a certain hour the judgment goes forth: 'This disease shall depart, and the patient shall live.' And forthwith the sentence is obeyed—the disease vanishes. That is, or was, the idea of a crisis; and it was further observed to be accompanied in many cases by certain evacuations, which were called on this account *critical*, and supposed to be, in fact, the means by which the disease, or the material cause of the disease (*materies morbi*) was carried off or expelled from the body. I am not concerned at present either to attack or to defend this old pathology; but there can be no doubt that this idea of a crisis harmonized well with the facts of many fevers as observed by those ancient physicians. But in typhus fever, as we see it at present, there is absolutely no trace of a crisis of this rapid and decisive kind. The fever is not, apparently, judged or condemned, and forced to vanish suddenly, after the manner of an evil spirit fleeing from the sentence of an invisible power. Nor does the resolution of the fever almost ever take place in company with marked evacuations, such as could possibly have suggested the idea of a purification of the system from a 'matter of disease.' On the contrary, the whole process is devoid of startling phenomena; it is gradual, distributed over days, and often for days imperceptible to all but the most minutely accurate observation; there is neither sweat, nor diarrhœa, nor any striking change in the urine; and if any of these should by chance occur, they are just as likely as not to be non-critical and of unfavourable import. This is no new doctrine as regards typhus fever, but it requires to be brought into stronger relief than in most of the current authorities; and the facts of this

particular epidemic, aided by the more general recollections of a good many years past, enable me to do this with perfect confidence as to the accuracy of the picture which I shall draw for your instruction, not from books, but from direct observation. For although the disease with which we have had to do has no *crisis* in the ancient sense of the word, it has nevertheless had a very regular and *normal course of increase and decline*, to which I now invite your best attention. The character of the disease has been, that it rises, often with perfect regularity, though quite gradually or even imperceptibly, to what may be called an *acmé*; and then, with no less regularity, and not less gradually, declines towards the state of complete convalescence, often without any evacuations or sudden startling phenomena of any kind throughout. The rise and the fall of the fever are equally gradual; and the curve indicating the fever—if one might represent it roughly (but still truly) in the form of a diagram—would not be after the ancient idea of crisis, thus :



but rather thus :



an *acmé*, or single summit, or maximum point of development, being a characteristic of both the diagrams; but the one which represents typhus differing from the ideal one of a crisis in the perfectly gradual character of the decline. From this normal type I hold that typhus fever very rarely departs, unless disturbed either by complications or by injudicious treatment; and therefore it is literally of immense importance to your comfort and security in treating the fever, as well as to the patient's security, which is of more importance even than yours, that you should know accurately the rules by which you are to watch for this slow and gradual crisis.

"Now what are you to look to with the view of catching the first glimpses, the first favourable indications, of this important movement? I believe that, speaking practically, and speaking of the great mass of cases, it is *the pulse*. The internal heat, as determined by the thermometer, is, I believe, a very valuable indication; but the thermometer is so much more cumbrous as a means of investigation, so much more difficult to apply, and takes up so much more time in the case of a busy practitioner having many cases to observe, that I believe the oldest of all studies in fever, that of the pulse, will hold its ground practically, at the bedside, and in ordinary investigations, against every other. It is easy, however, to study pulses in fever, without arriving at an idea of how valuable

and delicate a test of the crisis we have in the pulse, when watched for days together, and in connexion with an estimate, as accurately made as possible, of the duration of the disease. Of this I am sure, because I have again and again had my own observations criticised and doubted by good observers, and have had to come back to nature again and again to establish to my own satisfaction the points to which I have now to direct your attention, as regards the pulse-rate in typhus. What I have now to tell you has been carefully tested by the facts of the present epidemic, but is also consistent with facts observed by me on various occasions, at least since the year 1857, and particularly referred to in a publication of the year 1859, on the history of epidemic fever in Edinburgh.

"There is one stipulation I must make with those who desire to follow out this inquiry with the view of testing the normal mode of the crisis in typhus fever. It is, that *as many cases as possible should be left to their natural course, unaffected by either drugs or stimulants*. Of course, I should never think of recommending to you this mode of proceeding unless I believed firmly that the disease could be so treated in many cases with advantage to the patient, as well as with increase of knowledge to the physician. But this is precisely what I do believe most firmly, and what I mean accordingly to occupy another lecture in impressing upon you by arguments drawn from experience. Meantime let me state, as matter of opinion, that not only am I convinced of the safety and expediency of leaving many cases of typhus to take their *normal* course, but I further believe the normal course may be very easily altered for the worse by what is called treatment; and in particular, as regards the period of the crisis, I believe that the habitual or constant exhibition of drugs and stimulants has a great tendency to mask the disease, to disturb or to retard the crisis, and by so doing to increase the mortality. This is an opinion formed after a most careful observation of particular cases in detail over many years; and I advance it now, instead of waiting till next lecture for its fuller development, chiefly in order to add, that you cannot possibly observe the natural course of typhus unless you can make up your mind beforehand to leave many cases to follow their natural course. I am fully convinced that there are many practitioners who scarcely ever see a normal case of typhus, owing to their perpetual and systematic interference by drugs and stimulants; and it is even a question with me if the written descriptions of previous epidemics have not been largely vitiated by this cause, the disease being to some extent, as it were, disguised, or perverted from its natural and favourable course, by the treatment. How far this may have been so in fact I have no means of knowing exactly; but I have had abundant opportunities since the year 1857 of ascertaining that typhus fever, both in Edinburgh and Glasgow, has followed the normal course now about to be briefly indicated. . . .

"Now I venture to assert—and I make this assertion not by way of theory or opinion, much less prediction, but as a statement of facts, founded on a rather extensive experience ever since 1855 or 1857—I venture to put it forward as a law, or generalization, with respect to typhus fever: *That in a large proportion of cases, typhus*

fever, left to its natural course, and treated with abundant milk diet, and without drugs or stimulants, will have its natural crisis before the twelfth day. Of course you will understand that I use the word *crisis* in a modified sense; for I have just been explaining that there very rarely is in typhus a true crisis, in the ancient sense of the word. What I mean is, that most commonly before the twelfth day, very frequently as early as the ninth or tenth day, sometimes, though rather rarely, even before the ninth day, indications are to be perceived that the disease is about to terminate favourably; and that these indications are to be trusted, even when some of the most formidable-looking symptoms continue present to a later period. And this I confidently assert to be a very general law, or *norm* (to use a word borrowed from the Latin, but not yet naturalized in English, except in the adjective form *normal*), of typhus fever as observed in Edinburgh or Glasgow of late years. Observe, I do not venture to assert that it has always been so, or will always be so; on the contrary, I have myself remarked the tendency to a delayed crisis as an epidemic fact at certain seasons; but in a large proportion of cases, and excluding certain exceptional groups of cases, it has been so, in the main, in every year from 1857 to 1862 in Edinburgh, and during a large portion of the epidemic of the last two years in Glasgow.

"If you wish to have the satisfaction of getting the earliest information of this early crisis in typhus, *watch carefully and constantly, from day to day, or several times a day if need be, the rate of the pulse.* For this is, speaking practically, the guiding clinical fact in the natural history of typhus fever. Making allowance for certain obvious, and a few not so obvious, causes of accidental disturbance, you will find that the pulse pursues a very regular course of rising frequency up to the acmé, and declining frequency beyond the acmé, of the fever; and that although the other symptoms very surely follow the pulse in uncomplicated and regular cases, yet they often follow at some distance behind; so that the crisis can be ascertained for practical purposes, and particularly for the great purpose of guiding treatment, more accurately and effectually by frequent noting of the pulse-rate than by any other method. I have ample proof of this in the cases of the last two months, for which I will refer you to the hospital case-books, and to the abstract in the *Glasgow Medical Journal* for January, 1865.

"This accurate and frequent noting of the pulse-rate in typhus, in connexion with the day of evolution of the disease, is of immense importance as regards the prognosis; for not only are you able to predict the recovery with greater confidence, and at an earlier period, than you could do otherwise, but you have often the first intimation of complications, either during the fever or during the convalescence, through some unaccountable departure from the *norm*, or regular gradual increment and decrement, of the pulse. In fatal cases, where the death is from *pure* typhus, the pulse usually becomes more and more rapid, and at the same time weaker and smaller, up to the very hour of death. In complicated cases, where the complication is not very dangerous, the pulse may be merely

quicken for a day or two, or the crisis may be protracted, or the pulse may begin to rise again, after a quite distinct critical subsidence. In very dangerous complications the pulse is out of all bounds, and no rule can be assigned for its irregularities. But in regular and normal cases, being uncomplicated—*i. e.*, not having any positive or inflammatory complication—the decline of the pulse, even by a few beats, before, at, or about the twelfth day, carries as an after consequence the decline of the fever; and, on the other hand, a delay of the crisis very much beyond this is to be regarded with suspicion, though not by any means necessarily of fatal tendency.

“But the most curious inference which I have been able to draw from the cases of typhus which we have been observing for the last two months is this:—If in a particular case, you observe that the pulse keeps steadily to the *norm* or law of uncomplicated typhus, as I have endeavoured to exhibit it; if the rate of the pulse increases steadily and gradually up to the ninth, tenth, eleventh, or twelfth day, and then, at any period between the eleventh and fourteenth day is observed to be undergoing a correspondingly regular decrease, not rapid, but regular and gradual; *in such a case of regular crisis indicated by the pulse-rate, it is almost insignificant, as regards the prognosis, that the other symptoms continue formidable-looking for days*; for in all such cases that I have observed, without exception, the patient has got well in the end, and that without further treatment. Thus I can tell you from experience that, after a regular crisis of this kind, the tongue may be as black as pitch, and so dry that it feels like a bit of roasted leather, delirium may go on, the patient may be out of his bed many times in a night, the skin may be hot and dry, the eruption brilliant or livid for days together; still, if you have the favourable state of the pulse, and if there is no complication, *the patient will get well*. The pulse is your criterion. Whenever it is fairly turned, and declining in rate, especially if it is at the same time gaining in strength and volume, you may be at your ease, unless there is reason to suspect a complication; and even then you may be at your ease, if the complication is not severe. But observe, *only if you do not interfere*. The proper course in such a case is to take confidence from the pulse, and avoid interference. *You can afford to wait*. And only one more word to-day, but it is a very startling one, and yet true according to my experience: even if the delirium should become worse after the crisis, as indicated by the pulse; nay, more, even if the patient has had little or no delirium before, and *begins* to be delirious for the first time after the pulse has begun to show symptoms of crisis; still, it is better to wait, for in all probability he will be quite well in a few days, provided you do not disturb the course of the fever.”

ART. 23.—*On the Necessity for Milk in place of Wine
in the Treatment of Fever.*

By Dr. GAIRDNER, Professor of the Practice of Physic in the
University of Glasgow.

(*Lancet*, Jan. 21, 1865.)

Dr. Gairdner expresses himself very strongly upon this subject :—
“ You must feed your patients, and you must feed them chiefly on milk. Milk or buttermilk is, with me, the staple food of typhus, and I will even say that I know no other food that can be depended on. Yet I see, and always see with a new surprise, descriptions of the treatment and dietetics of fever, in which not a word is said about milk, and a great deal about beef-tea, wine, whisky, brandy, and all manner of things supposed to be more strengthening or stimulating than milk diet. Now, I tell you frankly that treating fever patients without plenty of milk is a thing that I do not understand at all, for I suppose that I have not treated a single case of fever of any kind for the last fifteen years (I cannot make precise statements beyond that date) without milk, and I always proceed on the understanding that milk in fever is the one thing needful as diet—always to be given, and given liberally, whether specially ordered or not. To give wine and whisky and beef-tea, while withholding milk, is simply, in my opinion, to destroy your patient; and the more wine or whisky you give while withholding milk, the more sure you will be to destroy your patient soon, because you are thereby superseding the natural appetite (or what remains of it) for a nourishing and wholesome diet, by a diet, if it can be so called, which poisons the blood and checks the secretions, and alters for the worse the whole tone of the nervous system, and of the digestion and assimilation. I believe that infinite mischief has been done in typhus fever and in all fevers by giving wine and withholding or not giving milk. Under a false theory of administering alcoholic food it has resulted, not only that natural and genuine food has been withheld, but that the small amount of appetite remaining for such food has been obliterated, and not unfrequently, even at an early stage of the disease, the patient has been practically disabled from taking any proper nourishment at all. I know unhappily as a fact, that not only doctors, but patients and patients' friends, are readily brought under the influence of this fatal delusion; that alcoholic liquors can, in fevers, take the place of natural food. But it is none the less a fatal delusion, and I warn you solemnly against it; all the more, that your patient is absolutely in your hands, and you can obtain no guidance from his natural instincts, if you begin by overwhelming them with alcoholic stimulants. I have been very careful, at least for fifteen years past, to avoid this error, and I believe that any success I may have had in managing fever, has been more due to this than to any other cause.”

ART. 24.—*On Fatal Hæmorrhage from the Bowels in Typhoid Fever.*

By Dr. PEACOCK, Physician to St. Thomas's Hospital.

(*Lancet*, February 4, 1865.)

During the whole summer and autumn of 1864 typhoid fever has been unusually prevalent, and the disease has presented certain features which are somewhat different from those which commonly characterize it. The attack was often more severe, and the early course of the disease more rapid, than is usually the case; the characteristic eruption was of more constant occurrence; diarrhœa was frequently absent, and in some cases the bowels were confined during the whole course of the attack; and yet hæmorrhage from the bowels appears to have been more frequent and more serious than it generally is.

"During the summer of 1864," writes Dr. Peacock, "I lost a patient from the immediate effect of a copious hæmorrhage; a second similar case occurred at the same time in the hospital, under Dr. Barker; and about a month ago I went down into the country to see a patient suffering from typhoid fever, and who, the day after I saw him, was taken ill with hæmorrhage, under which he sank. I have also heard of several other cases in which large losses of blood occurred in typhoid about the same time, though the patients recovered. Of the three cases referred to I will give brief abstracts.

"The case which fell under my own notice was that of a female, aged twenty-seven, who was admitted into St. Thomas's Hospital on the 2nd of July. She stated at the time of her admission that she had been ailing for about three weeks, but seriously ill for only two days. She was somewhat prostrated and torpid; the pulse slightly accelerated; the tongue covered with a whitish-brown fur; and a few spots, rose-coloured, slightly elevated, and distinctly typhoid, were situated on the thorax and abdomen. There was no pain or tenderness on pressure in any part of the abdomen, and the bowels were acted upon naturally once daily. On the 4th she was more prostrated, and there was considerable torpor of mind; the tongue was dry and brown, but the bowels still acted naturally. Two days afterwards—the sixth day of active illness—she suddenly passed, at seven A.M., about a pint of blood, and discharges continued at intervals, so that she voided altogether about two quarts. She rapidly became collapsed, and died at thirty-five minutes past five P.M.

"On examination after death no material disease was found in any part of the body except in the alimentary canal, and the changes were there limited to the lower portion of the ileum and the cæcum and colon. Those portions of the bowel contained a large quantity of blood, partly fluid and partly in dark-red coagula. Several of the patches of Peyer displayed thick masses of typhoid deposit, and the mucous membrane over them had in some places just begun to ulcerate. Near the cæcum there was a large patch with a portion of the mucous membrane sloughing, and a similar slough existed on the

ileo-cæcal valve. The colon contained a few small masses of deposit, one or two of which were beginning to ulcerate; and the follicles in the colon were unusually distinct. A small cyst in the ovary was full of blood and pus; there was a recent corpus luteum, and the uterus was retroverted.

"Dr. Barker's patient was a girl, fourteen years of age, who was admitted into the hospital on the 5th of July, 1864, and presented symptoms of typhoid fever, under which she had laboured for nine days. The symptoms commenced gradually, and she had been confined to bed a week. There was no eruption on the skin, and the bowels were not relaxed. She was quite rational, and the symptoms indicated only a slight attack.

"On July 7th, at seven P.M., she passed blood from the bowels, and shortly afterwards three other evacuations of almost unmixed blood were voided. Altogether she lost from three to four pints, and sank rapidly, dying at a quarter to ten the same evening.

"On examination, the mucous membrane of the ileum was somewhat congested, especially at its lower part, and for the space of about a foot from the cæcum it contained numerous typhoid deposits in the agminate follicles, over many of which the membrane was entire; while in others it had ulcerated, in some over a large portion of the patch; in one or two places the ulceration extended nearly to the serous coat. The large intestines were healthy, the disease ceasing at the ileo-cæcal valve. The intestines contained very little fecal matter. The spleen was congested, and weighed nine ounces. The other organs were all healthy.

"The notices of the post-mortem examinations in these cases are taken from the notes of Dr. Hicks, the demonstrator of morbid anatomy and curator of the museum at St. Thomas's.

"For the particulars of the cases which I saw in the country I am indebted to Dr. Wyman, of Hatfield Broad Oak. The subject of the case was a clergyman, thirty-eight years of age, who had been out of health for some months, and had come recently from Lancashire, where he had had much work and anxiety during the cotton famine. He had sometime previously had an attack of rheumatic fever.

"The patient was first seen by Dr. Wyman on October 3rd, and then complained of general indisposition and feeling of languor. His tongue was coated and the bowels confined. A mild aperient was given to him, and he was directed to take some quinine and acid. He, however, got worse, and had at intervals attacks of sickness.

On October 12th, or the ninth day of illness, some rose-coloured spots were observed on the abdomen, examination not having been previously made.

"On the 14th, severe diarrhœa occurred during the night; but this was checked, and for several days no fresh spots appeared, and he was upon the whole better.

"On the 26th, or twenty-third day of illness, he was taken much worse: the sickness again came on, his pulse became more frequent and the tongue dry and brown, and fresh spots made their appearance. He continued to get worse, suffering at intervals from sickness and diarrhœa.

"On the 5th of November, or the thirty-third day of illness, I saw him with Dr. Wyman, and Mr. Scarr of Bishop's Stortford. He was then quite intelligent, but had an oppressed look, and spoke in a slow, somewhat hesitating way, which, however, I was told was natural to him. His tongue was dry and morbidly red; his pulse not quick, and of fair volume; and the bowels were acted upon two or three times daily. He was, however, much prostrated, and had passed one or two motions in the bed, not from want of consciousness, but from want of power over the sphincter. There was also considerable tympanitic distension of the abdomen, and some tenderness on pressure. There were several typhoid spots on the skin. He had been taking tonics and stimulants, with astringents and opiates to control the diarrhoea, and the same system was decided to be still carried out.

"The following day he was worse, and at eight P.M. he began to pass large quantities of blood by stool, and this continued at intervals all that night and through the next day till half-past three P.M. on the 8th, or thirty-sixth day of illness, when he died. No post-mortem examination took place.

"The cases which I have related are, I believe, instances of an unusual occurrence in typhoid fever. I have generally regarded the passage of small quantities of blood during the course of the disease as not of much importance; and I have not previously seen any cases in which the patients have died from the direct effect of the hæmorrhage, though several have fallen under my notice in which large quantities of blood have been passed. Indeed, of the only four cases of which I possess notes, in which patients who have had copious hæmorrhage from the bowels have subsequently died, two died during advanced convalescence, and from causes having no connexion with the hæmorrhage; while in the other two the bleeding could only indirectly have conduced to the fatal result.

"It will be observed that the cases in which the fatal hæmorrhage occurred varied considerably in severity. In my own case, the symptoms during life were not such as to indicate that the attack would prove a severe one; and after death the disease in the intestines was not extensive, there being but few plates in the ileum which were enlarged, and only two which were sloughy; while in the cæcum only a few follicles were enlarged and beginning to ulcerate. In Dr. Barker's case, the symptoms also during life were not urgent; but the disease in the intestines was much more considerable, though limited to the lower portion of the ileum; there being numerous ulcers of the agminate glands, some of which penetrated deeply into the intestinal coats. In Dr. Wyman's case, the disease in the early stage was rather prolonged than urgent; but in the relapse it became more severe, there being vomiting, diarrhoea, and involuntary discharge of the stools. I believe the occurrence of urgent sickness and vomiting in the early stage of typhoid may, as in scarlatina, be regarded as indicating a severe form of the disease. In all the four other cases to which I have referred, though the patients recovered from the fever in two instances, and died at late periods after the hæmorrhage in the other two, the attacks were

severe. The period at which the bleeding occurred varied considerably in the different cases, but in all it took place during the active progress of the intestinal disease. Thus in my own case the hæmorrhage took place on the sixth day of active illness, and the patient died in ten hours and a half; in Dr. Barker's case the bleeding came on on the eleventh day of the fever, and the patient survived for scarcely three hours; and in Dr. Wyman's case the hæmorrhage occurred on the thirty-fourth day of illness, and the patient sank in forty-three hours and a half. Of the four other cases mentioned, in one the discharge of blood occurred on the eighteenth day of fever, and the patient survived four days; in the other, the bleeding took place on the twenty-fourth day, and the patient died on the thirty-sixth. In the two cases in which the patients did not die in any degree from the hæmorrhage, the patients had the bleeding early in the attacks, and died, one after an illness of seven weeks of arachnitis, originating in otitis, and the other after eight weeks of pleuritic effusion. In the former case, the patient had been discharged from the hospital as cured; in the latter, he was only admitted when the pleurisy had commenced.

"In two of the cases last named, the patients passed bloody urine as well as discharging blood by stool, and in a third there was copious epistaxis, and blood was also discharged from the ear. Dr. Wyman, in reporting the former case, says that about the same time he had another case of typhoid under treatment, in a boy eight years of age, who both passed blood by stool, and vomited blood. It is well known that epistaxis is of frequent occurrence during the early stages of typhoid. I have known large losses of blood very rapidly take place, and the patient be so placed in imminent danger; and a case in which a large loss of blood occurred from the nose fell under my notice in the hospital a few months ago. This frequent occurrence of hæmorrhage from other parts in typhoid, and the discharge of blood from several surfaces at the same time, indicate that the intestinal hæmorrhage is not due simply to the erosion of some large vessel, but depends, in part at least, upon the altered qualities of the blood under the influence of the fever poison. The same was a not unfrequent source of danger in the relapsing fever of Edinburgh in 1843, in which disease there was no intestinal affection, and often nothing to be detected in the bowels after death, except some congestion of the mucous membrane. It is curious that hæmorrhage from the mucous membranes should be so common in these two forms of fever, while subcutaneous hæmorrhage is rare in them. In typhus, on the contrary, intestinal hæmorrhage seldom, if ever, occurs, though blood is occasionally found extravasated internally, as on the membranes of the brain or in the abdominal muscles; and yet the eruption which appears on the skin becomes always more or less petechial towards the latter period of the disease, and often there are large ecchymoses, especially on the dependent parts. In the two cases in which the bodies were examined after death, the precise source from which the discharge of blood had occurred could not be detected, and it is very possible that no very large vessel had in either case been eroded."

ART. 25.—*On the Prevalence of Scarlet Fever in England.*

By Mr. J. N. RADCLIFFE, Honorary Secretary of the
Epidemiological Society.

(Proceedings of Epidemiological Society, *Social Science Review*, April, 1865.)

The Registrar-General's returns of scarlet fever, for the whole of England, include two periods of five and sixteen years respectively. The first period extends from 1838 to 1842, and the second from 1847 to 1862, inclusive. The total number of deaths registered from the disease in the twenty-one years was 310,720; the annual average mortality for the whole series of years was 14,796. If a comparison be instituted between quinquenniums, it is found that, in the first (1838-42), the average yearly mortality was 12,582; in the second (1847-51), it was 15,065; in the third (1852-56), 16,720; and in the fourth (1857-61), 14,089. Or, to make a more accurate comparison, in the first quinquennium the yearly average mortality per 100,000 population, living at all ages, was 81; in the second quinquennium, 84; in the third, 89; and in the fourth, 74.

It is thus seen that, in the second and third periods, there was a great increase in the mortality from scarlet fever; in the fourth period a considerable falling off. The relationship of this decrease to the outbreak of diphtheria is of peculiar interest, as will presently be seen.

Four times in the twenty-one years scarlet fever was epidemic. The annual average mortality, per 100,000 population, during the whole period, was 82. The first epidemic occurred in 1840, when the death-rate reached 128—the highest mortality which took place from the disease in the twenty-one years. In the year following (1841) the mortality was above the average. The second epidemic occurred in 1848, when the death-rate was 118. In the year previous the mortality had risen slightly above the average. The third epidemic occurred in 1852, when the death-rate reached 103. In the three following years, 1854, 1855, 1856, the mortality continued above the average. The fourth epidemic occurred in 1858, when the death-rate was 121—the highest which had happened since 1840. In the year following (1859) the mortality averaged 97 per 100,000 population.

Diphtheria became epidemic in 1858, the year of the fourth epidemic outbreak of scarlet fever. In 1859 the epidemic of diphtheria reached its maximum, scarlet fever still being more than ordinarily prevalent. In 1860 diphtheria continued epidemic, but scarlet fever had become comparatively little prevalent, or its fatality had decreased so greatly that the mortality from this cause was less than half that which occurred in 1859. In 1861 the mortality from diphtheria exceeded that of the first year when the disease became epidemic, while the mortality from scarlet fever was but little more than half the usual average. In 1862 the mortality from diphtheria still continuing high, that from scarlet fever had increased to a tenth below the annual average. In 1838 the mortality from scarlet fever averaged 39 per 100,000 population; in 1860, 46; and in

1861, 45. These are the three years of lowest mortality from the disease recorded.

The outbreak of diphtheria contemporaneously with one of the greatest epidemics of scarlet fever which has prevailed in England, and the subsequent declension of scarlet fever contemporaneously with the apparent naturalization (so to speak) of diphtheria among the population, are facts of great interest. Scarlet fever has prevailed epidemically at the same time with diphtheria in other countries, but facts are still wanting which would enable us to explain the concurrent prevalence of the two disorders. It is probable, however, that the fluctuations of scarlet fever from 1857 to 1862 are not greater than those which have hitherto been observed in the progress of the disease. In other words, it is probable that the diminution of the disorder in 1860-61-62 is but a temporary phenomenon, having no necessary, or even intimate, connexion with the existence of diphtheria. In 1858 scarlet fever was more fatal and prevalent in London than it had been in any year since the weekly bills of mortality had become trustworthy. In 1860 the deaths from the disease fell below the average. In 1861 the malady again became epidemic in the metropolis, and in 1862 the mortality from it was, with the exception of the epidemic of 1840, the greatest which has ever been recorded from this cause within the bills of mortality. It is far from improbable that scarlet fever was excessively virulent throughout England during 1863.

The history of the progress of scarlet fever in the metropolis differs from that of the entire kingdom in this, that it shows a great augmentation of the mortality from the disease in the last quarter of a century. The annual average mortality from the malady in London during the past twenty-six years was 83 per 100,000 population. The average varied from 32 in 1841 to no less than 174 in 1863. In the quinquennium 1839-43 the annual average was 78; in the quinquennium 1844-48 it increased to 88; in the quinquennium 1859-63 it advanced to 115. The death-rate of 1863 (174) was more than double the annual average of the twenty-six years 1838-64.

Scarlet fever was epidemic in London in 1839-40; 1848 (when the death-rate reached 212, nearly twice the annual average), 1852, 1854-55, 1858-59, and 1862-63. In fact the disease was epidemic in London at the same time as throughout the entire kingdom, except in 1862. In that year the mortality throughout the country was much larger than in 1861. It remains to be seen whether the parallel holds good of 1863.

ART. 26.—*On the Limitation of Scarlet Fever.*

By MR. J. N. RADCLIFFE, Honorary Secretary of the
Epidemiological Society.

(Proceedings of Epidemiological Society, *Social Science Review*, April, 1865.)

Mr. Radcliffe, after having animadverted upon the little attention given to the limitation of scarlet fever in the chief text-books of medicine, discusses the probable sources of distrust in the means of

prevention at our command, protests against the neglect of these means, and continues:—

“I speak hopefully of our power in limiting the ravages of scarlet fever. And yet of the means by which this is to be effected, I have but an old story to tell. I cannot suggest any new means of prevention, but I have no lack of confidence in those already in use. What is wanted is the more steady and persevering use of these means, and a provision for their application to the great foci of infection—the crowded courts of great towns. Both requirements are feasible.

“Withering, in the past century, clearly laid down the true prophylaxis of scarlet fever. I omit the fanciful measures based upon his theory of the mode in which the infection of the disease is conveyed into the system. ‘It is of peculiar importance,’ he says, ‘also to know that the progress of the infection may be stopped by the use of very practical precautions, such as may be adopted in almost every house. When it first appeared among us, it often ran through whole families, and in boarding schools particularly, it made such havoc that most of the schools in the town and vicinity of Birmingham were under a necessity of dispersing; and the yet healthy children of many families were sent home; but this method contributed to spread the infection more widely and more speedily through the country.

“‘From the time Dr. Haygarth first communicated to me his ideas of stopping the progress of the small-pox, the probability of stopping the progress of scarlet fever by the adoption of similar methods was too evident to escape the most inattentive observer. The first trials proved successful; and the full body of evidence, elucidated by the clearest reasonings, which appeared soon afterwards from the same masterly hand, encouraged me to proceed; and now for several years past, I have never thought it necessary either to break up a school or to disperse a family. Allotting apartments on separate floors to the sick and to the healthy; choosing for nurses the elder parts of the family, or such as had the disease heretofore, and prohibiting any near communication between the sick or their attendants and the healthy, with positive orders to plunge into cold water all the linen, &c., in sick chambers, have very universally been found to check the further progress of the infection.’

“Dr. Richardson, writing two years ago, insists on the carrying out of a stringent quarantine. ‘Dr. Blackburn,’ he says, ‘half a century since, entirely proved that, even in a boarding school, the disease would not spread if the patients were confined to one room, and the attendants were kept altogether away from the infected. Nor should the separation of the patient be confined merely to the time when the acute symptoms are being manifested; *it should extend to at least a month after the date of the commencement of the attack.* In the second place, the room of the patient should be kept sedulously ventilated, as well for his own safety as for that of others. Lastly, as it is obvious that almost every material object in contact with the patient is capable of holding the poison, all articles of clothing and furniture should be thoroughly disinfected

by the best of all disinfectants, heat at boiling point, before the susceptible body of another person approaches them. With strict adherence to these simple rules, this disease, according to my experience of it, can be kept safely from propagation.'

"My own experience largely coincides with that of Dr. Richardson. But the difficulties to be contended with in securing an effective quarantine must not be underrated. Nothing but the extremest tact and care can overcome them, or exclude all probable modes of propagation of the infection. At times, even our best efforts may be unavailing. But this does not militate against the advisability, nay, the imperative necessity, of such efforts.

"In securing an effective ventilation of the room occupied by the patient, and preventing the formation of dépôts of the poison, it is well to insist upon all superfluous articles of furniture, including carpets and curtains, being removed. These should be thoroughly cleaned and ventilated before being stored up or placed in other rooms. I am in the habit of advising whitewash or fresh paper to the walls, and paint to the woodwork, after the cessation of the disease, when practicable. When not practicable, and when the condition of the walls and ceilings renders their being fitly cleaned somewhat doubtful, it is well to inundate the room for some time with nitrous acid or chlorine gas. Again, if a thorough ventilation of a room cannot, as is too often the case, be obtained, it will be advisable to make frequent or constant use of chlorine or nitrous acid, given off in small quantities, so as not to irritate the patient's lungs. The vapours of iodine also may be used as recommended by Dr. Richardson.

"Frankly, I have a distaste for gaseous disinfectants, unless they are absolutely required. They are too commonly made a plea for the neglect of the more important measures of cleanliness and ventilation. The majority of people are apt to place their chief reliance on these disinfectants. They are dangerous adjuncts to prophylaxis on this account; hence I never use them if I can properly avoid so doing.

"But there is a class of cases which, from the nature of the patients' homes, and the impoverishment of their families, cannot be dealt with in the manner above indicated. Neither separation, nor cleanliness, nor disinfection, can be obtained for them. These cases, which always exist more or less in great towns, are the persistent sources of infection in the kingdom. Any scheme of limitation which does not provide for them, of necessity fails in its most important part. Withering had pointed out the probability of stopping the progress of scarlet fever in the same manner as the progress of small-pox might be stopped. This, it will be remembered, was anterior to a knowledge of vaccination. The full significance of Withering's remark does not, until lately, appear to have been rightly understood. The credit of giving that development to the suggestion of which it is capable, belongs, as far as I am aware, to Dr. Morgan, the honorary secretary of the Manchester and Salford Sanitary Association. In his report on the health of

Manchester and Salford in 1863, he makes the following observations in reference to scarlet fever:—

“We should consider, inasmuch as the disorder is even now raging, what steps can be taken by the Sanitary Association with the view of settling some barrier to its onward march. And here let us recall to mind the course of action adopted some twelve months ago when a similar danger impended from the small-pox. At that time the Sanitary Association not only called upon the authorities to use their best efforts in enforcing the removal of the sick from the abodes of the healthy, but also, through the public press, made known the reality of the peril and the best method of checking its encroachments. As a consequence of the timely warning thus raised, no fewer than 331 patients were removed to the public workhouses’ fever-wards; and, inasmuch as the total number of seizures in the course of the year did not exceed 1269, 1 in every 3·8 were separated from their relatives, and thus prevented from becoming so many fresh centres of infection. When, however, we turn to inquire how far this attention to the isolation of affected persons has been observed in respect to scarlet fever, we find that in the course of the year only 28 out of 1927 patients were removed to the workhouses and fever hospital. To the encouraging of such separation of the sick, the Sanitary Association will doubtless direct its best efforts. The object may probably be promoted by addressing the boards of guardians on the subject; and, likewise, by pointing out to the managers of schools and of mills the great danger attending the admission of children from houses in which the disorder is known to prevail, and by not allowing the convalescent to return to their work for some weeks subsequently to their recovery. Such precautions, at the present day, are constantly observed in the better regulated schools throughout the country; and considering the heavy issue at stake, the hardship of enforcing them on the poor would not be oppressive. These remarks naturally apply solely to the poorer portion of the population—those, in fact, who are dependent on charity for their treatment in sickness. But the question arises as to whether, when epidemics are raging, it might not be expedient to encourage the establishment, in various quarters of the town, of self-supporting institutions, in which, on the payment of a weekly fee, the children of artisans and of such persons among the middle classes as might desire to avail themselves of their benefits, might obtain admission. Such institutions should be situated in those quarters of the town in which the disease was observed to take a mild type. By such an arrangement those dwelling in localities less favourable to the disorder would, by the circumstances of removal alone, reap advantage. In many cases, also, in which at present every member of a family is successively attacked, the immediate removal of the first seizure might arrest the disease at its source.”

“The suggestion here made seems to me largely to meet the great difficulty which has to be contended with in the limitation of scarlet fever. It is a suggestion capable of wider extension than Dr. Morgan has given to it. I think that, in all the great centres of the activity of the disease, special public provision should be made for

the segregation of the sick from scarlet fever, in connexion with the hospitals or workhouses, or both. The provision thus made should be eminently practical and simple. Inexpensive, but durable and well-formed buildings should be erected. Such buildings being in existence, cases of infectious disorder could be weeded out of the dense masses of the pauper and operative population as they occur, at an immense gain to the people, and with little pressure on public charity and the rates. Many stumbling-blocks may be in the way of these suggestions, but none which are to be looked upon as insuperable."

ART. 27.—*Permanganate of Potash as a Remedy for Diphtheria.*

By LOUIS MACKALL, Jr., Georgetown, D. C.

(*American Journal of Medical Science*, January, 1865.)

"After using faithfully all the remedies both general and local which have been extolled for the cure of diphtheria, and having seen so little good result from their use," says Dr. Mackall, "I had lost in great measure all faith in such remedies, and had come to the conclusion that the best treatment was to support the patient with nourishment and the free use of stimulants. On reading, in the January number of the *American Journal*, an article by Dr. Samuel Jackson on the therapeutical application of a solution of the permanganate of potash and of ozone, it occurred to me that this agent might be beneficial in the treatment of diphtheria. Shortly afterwards I had an opportunity of making a trial of it in a severe case. A young girl about eleven years of age was seen by me after being sick several days. The tonsils, soft palate and fauces, were covered with an ash-coloured deposit; the glands beneath the jaw were much swollen, with frequent pulse and hot skin; she was treated for several days with chlorate of potash and the tincture of the chloride of iron. Muriatic acid and tincture of iron in equal parts, were applied locally. But finding the disease on the increase, I changed this treatment and used the permanganate of potash both internally and as a local application, the latter in the proportion of 3j to water Oj. She took a teaspoonful every three hours of the strength of 3j to water Oiss. On the second day after beginning this treatment, the improvement was very marked, and she speedily recovered. The false membrane was detached and the mucous membrane presented a healthy appearance in three or four days.

"Since then I have treated all the cases of diphtheria (some fourteen or fifteen) which I have seen with this agent, and am more and more convinced with every case, that we have in the permanganate a most valuable remedy. Such is my faith in its power to arrest the extension of the pseudo-membranous formation, and to remove it when formed, that I now feel little apprehension in any case if called to see the patient before the disease has extended

to the larynx, or paralysis has occurred. Indeed, in those almost hopeless cases in which it is evident that the disease has reached the larynx, as shown by suppressed cough and voice with paroxysms of intense dyspnoea, I have seen under its use three children recover. With a considerable experience in the disease I had previously known only one child to recover under similar circumstances. These three cases were all of the most unfavourable character; the membranous formation was abundant; the laryngeal symptoms very distressing. In all of the cases I expressed a very gloomy prognosis, as all similar cases, with the one exception above mentioned, had proved speedily fatal. In these cases I also used emetics, but I think the successful result should be attributed to the permanganate, as I had used emetics in all such cases before without benefit.

"When the disease has extended beyond the reach of this remedy locally applied, of course a successful result could not reasonably be expected from its use; but I believe that with this agent we can prevent diphtheria from progressing to a fatal termination, provided the cases can be attended to before the larynx becomes involved.

"Its tendency to attack the mucous membrane of the pharynx prior to its extension to the larynx is characteristic of diphtheria, and I feel assured, from my experience, that if the permanganate of potash is used in this stage, that it will not only control its further development, but will speedily remove all traces of the disease by restoring the mucous membrane of the throat to a healthy state.

"The inferences that it is intended should be drawn from the foregoing remarks, are: that if diphtheria arises from a specific cause affecting the whole system, then the permanganate of potash may be regarded as the antidote to this poison; or if the fatal tendency is thought to be caused by, or to be dependent on, the local affection of the throat, then the local affection may be removed and the fatal tendency may be obviated by the use of this remedy.

"It may be well to state that I have never seen any unpleasant effect from the use of the permanganate, even when administered to young infants (the solution should be weakened by increasing the quantity of water to Oij to permanganate ʒj in very young children); and I have observed that when locally applied it causes less distress than almost any other remedy."

ART. 28.—*On the Treatment of Acute Rheumatic Fever
Exclusively by Free Blistering.*

By Dr. HERBERT DAVIES, Physician to the London
Hospital.

(Pamphlet, Churchill, 1864.)

Dr. Davies aims at eliminating the *materies morbi* of rheumatism at the inflamed joints, instead of neutralizing it in the system by alkalies or other medicines. He orders blisters, varying in width,

but of considerable size, according to the locality, to be applied around each limb, and in close proximity to the parts inflamed; and he hopes to relieve the affected joints, partly on the principle of derivation, but mainly and really by affording, through the serous discharge from the blistered surface, a ready means of exit for the animal poison. Twenty-five cases are given in illustration. We should have thought the patients would not submit readily to such a plan of treatment—that, in fact, the remedy was worse than the disease; but this is not the lesson taught by these cases. Dr. Davies says that convalescence is hastened, and the tendency to cardiac disease lessened, by this mode of treatment; but further evidence is necessary before this opinion can be accepted. Indeed, nothing is more uncertain than the value of any particular plan of treatment in rheumatic fever, for if all is to be believed that has been said of more than one plan of treatment, there is no necessity to concern ourselves with any new plan.

One of the cases given in illustration is the following—a somewhat extreme case, it is true :—

CASE. —A large-made, plethoric, beer-drinking carpenter, was admitted one Thursday evening with most severe articular rheumatism, affecting seven joints. The pulse was 120, and hard; the temperature of the left axilla as high as 102.5°; the urine, perspiration, and saliva intensely acid; thirst excessive; appetite nil, and tongue foul. He had had no sleep for four nights. Seven large blisters were applied next (Friday) morning, around each limb affected, and in close proximity to the inflamed joints. I saw that they were carefully put on. On the Saturday morning they were removed, and linseed-meal poultices kept on for sixteen hours. By this means a large amount of serum flowed away, and on that (Saturday) morning the pulse had fallen to 86, the axillary temperature to 99.5°; the urine had become *absolutely alkaline*, and although the thirst was still great, he had some return of appetite. He had lost all pain in his limbs, and his joints were freely moveable. On Sunday he was dressed and sat up for three hours. On Monday he was so far recovered as to be able to walk by the aid of a stick; and on Tuesday morning, on entering the ward, I found that he had risen and dressed himself of his own accord. The appetite was then good, and he declared himself to be only weak. He had eggs on the Monday, fish on the Tuesday, meat on Friday, and was discharged perfectly well on June 18th, after being sixteen days in the hospital.

His heart was perfectly sound at the time of his discharge.

Being an intelligent man, and accustomed from his business to measurement, I desired him to write me out a short account of his case, and to calculate how much blister-plaster had been applied to his skin. The following is his letter, from which it appears that, taking the mean of the thigh measurements to be sixteen inches, the number of square inches of blister-plaster amounted to 296½. No symptom of strangury appeared, and no inconvenience whatever beyond the pain to which he alludes. Their beneficial effect was rapid and complete, and their constitutional action, exhibited in the fall of the pulse from 120 to 86, and the production of *alkalinity* of the urine, as soon as full discharge of serum had been established :—

“June 16th, 1864.

“SIR,—The following is a true statement of my case, to the best of my

knowledge and belief. I entered the London Hospital on Thursday the 2nd day of June, and was treated in the following manner:—

“June 3rd.—Received application of seven blisters on different parts, viz., one on left shoulder, three by three inches and a half; one on each thigh, seventeen inches and a half at greatest length, and sixteen inches and a half at lesser length, with a width of five inches; one near each ankle, ten inches by four; one near each wrist, eight inches by three and a half. I received relief from rheumatic pains immediately the blisters began to draw. At midnight on the 3rd inst. they caused me much pain.

“4th.—The blisters were dressed at 4.30 A.M., and linseed-meal poultices applied, which were continued till 8 P.M., when they were dressed with zinc ointment. In the meantime the rheumatic pains had almost entirely left me.

“5th.—I got out of bed, and dressed with the assistance of a fellow-patient. Sat up three hours, the rheumatic pains having entirely left me.

“6th.—Dressed myself without assistance; felt rather stiff; was able to walk about the ward with a stick. Sat up five hours.

“7th.—Could walk about the ward without a stick.

“8th, 9th, 10th, 11th.—The skin having healed up, I could walk as well as ever. “I have the honour to be, Sir, your obedient servant,

“Dr. Herbert Davies.

“J. B.

“P.S.—The following particulars I forgot to mention in the preceding part of this letter:—

“I was attacked with pains in my left shoulder on Sunday the 22nd of May, but I did not know what was the cause; I thought it was only a cold. It got to my left leg at night. On the 23rd I got worse. I was told that I had the rheumatic fever. Had some medicine and a powder given me, and was ordered to apply hot bran poultices to my shoulder, and to bathe my ankles (where I felt most pain) with a solution of soda and water as hot as I could bear it.

“I got so much worse that I was compelled to lie in one position (on my back) for several days, the agony being intense. I remained in this state till the 2nd of June, when I was got downstairs with much difficulty, placed in a cab and brought here, and was received into Harrison's ward at 3.15 P.M.

“J. B.”

ART. 29.—*Acid Dyspepsia as a Symptom of a Rheumatic State.*

By Dr. HORACE DOBELL, Physician to the Royal Infirmary for Diseases of the Chest.

(*British Medical Journal*, March 11, 1865.)

The following quotation is taken from a clinical lecture on the forms and vestiges of rheumatic fever:—

“A very simple group of symptoms, monotonously similar in most of the cases, serves for a correct diagnosis.

“1. Pain at the epigastrium, coming on within an hour after food, generally within half-an-hour, very often within a few minutes, especially excited or aggravated by cheese or malt liquor.

“2. Urine very acid, of high specific gravity, high coloured, and depositing urates on cooling, and uric acid crystals after standing

some time; often suddenly changing to limpid and low specific gravity, but quickly returning to the other condition.

"3. Tongue red at the tip, with elevated papillæ; often red at the edges also.

"4. Appetite usually good, often very good, especially for animal food.

"5. Epigastrium tender on pressure.

"Such are the features of the majority of the cases, of course complicated and modified by the existence of other diseases, the habits of the patients, &c.

"The most marked of these is the time at which pain begins after food: it is very soon after. Sometimes we find an old neglected case in which the pain comes on later, even an hour and a half, and rarely, two hours after food; but if it be a case of acid dyspepsia, you will find, on close inquiry, that when the symptoms first set in, the pain was directly after eating; and that the time of its onset has become later and later. In these cases, too, the appetite will be bad, instead of good; and the food will often be vomited. They are, in fact, exceptional cases, in which the digestive power has become paralysed by the neglect of the original disease. With these exceptions, if the pain does not begin till more than one hour after eating, the case is not one of acid dyspepsia. To demonstrate these exceptions, I may mention to you Case 64,297, domestic servant. She complained of pain in the pit of the stomach, beginning from one to two hours after food, with much flatulence; but said that when it began, five weeks before admission, it came on much sooner after food; and as the complaint yielded to treatment, the pain came on sooner and sooner after food, till it ceased to come at all. In Case 64,525, the food was brought up undigested; and in Case 64,207, there was no appetite and no taste for food on admission; but when the complaint began, two months before, the appetite was very good.

"So much, then, for the symptoms of this complaint. The next point is as simple and as striking as the symptoms; viz., that the cases almost invariably get well in a few weeks under a very simple plan of treatment.

"1. We order them to take out-of-door exercise, and to attend to the condition of the skin.

"2. We forbid them to take malt liquor or cheese, and allow whisky or brandy instead; and advise water to be drunk freely.

"3. We neutralize the secretions by soda and potass in full doses, with lemon-juice or citric acid; then follow this with an alkaline stomachic powder of soda, potass, ginger and calumba before meals; and, if the patient be weak, give iron and quinine to finish. Sometimes we have to put a very small blister over the pit of the stomach, but not often. This alkaline plan of treatment must be thoroughly carried out; and it is surprising what large quantities of alkaline are often required. If you find the symptoms obstinate, you will be sure to find that the secretions are still too acid: they must be completely neutralized at the onset; and then you will quickly find the tongue lose its red tip and edges, and there will be no more pain after food. This feature in the treatment brings me to

the point upon which all the interest turns—the link which connects these common cases of acid dyspepsia with the cases of disease of the heart.

“It lies in the fact that this acid dyspepsia is a symptom of a rheumatic state of the system. If you inquire into these cases carefully, you will discover in the majority a rheumatic history. You will find that they belong to rheumatic families, or that they have themselves been the subjects of rheumatic affections. This fact is illustrated by many cases now under treatment.

“There is every reason to believe that the acid, which is found in such quantities in the stomachs of these patients, is either identical with or closely allied to the *materies morbi* of rheumatism—the acid which must exist in excess in the organism before a person can have rheumatic fever.

“Whether it is first generated in, or poured into, the digestive tract, and afterwards absorbed into the blood, may be open to question; but, whatever be the exact order of events, it is quite certain, from clinical observation, that it appears in the stomach very early in its career of mischief, and there gives rise to the symptoms of acid dyspepsia, which may, therefore, be taken as an important signal for alarm, never to be disregarded. It shows that the enemy is collecting his forces for an attack.

“For this reason, as you may have observed, I seldom discharge patients who have been under treatment for acid dyspepsia without giving them directions for their future management, and the prescriptions of their alkaline medicines to be resorted to whenever they find the symptoms of their complaint returning. By adopting this plan, I have known many persons keep free from rheumatic fever and rheumatism for years, who previously had been subject to periodic attacks at short intervals.

“If you inquire into the histories of persons who are subject to rheumatic fever, you will find, as a general rule, that they frequently suffer from acid dyspepsia; and, if you inquire into the histories of persons subject to acid dyspepsia, you will generally be able to make out either that they have suffered from some rheumatic affections, or that they belong to rheumatic families. In these two sets of cases, then, which are so numerous at this infirmary, we see the two ends of a long and sad story. So far as I am aware, the connexion between them has never before been specially pointed out; but I believe that I cannot too strongly impress upon you the importance of bearing in mind, when you get into practice, the fact that acid dyspepsia and valvular disease of the heart are to be regarded as the germs and vestiges of rheumatic fever; and that if you keep in check the acid dyspepsia, you will not have the rheumatic fever; and that thus you may prevent the occurrence of the valvular disease.”

ART. 30.—*Remittent Fever with Pityriasis or Erythema.*

By PROFESSOR C. A. WUNDERLICH.

Archiv. d. Heilkunde, vol. 1, p. 37, 1864.

CASE.—A WOMAN, TWENTY-SEVEN YEARS OF AGE, SICKENED ON THE 25th OF DECEMBER, 1862, WITH HEADACHE, PAINS IN THE FINGERS, SCAPULERS, ELBOWS, AND FEET, FEVER, AND SEVERE RIGORS. ON THE FOURTH DAY SHE SAW, ON THE CALF OF HER LEFT LEG, SOME SMALL RED SPOTS, THAT INCREASED IN THE FOLLOWING DAY, AND APPEARED ALSO ON THE RIGHT THIGH, BREAST, AND ARM. ON THE MORNING OF THE SIXTH DAY THERE WAS HIGH FEVER TEMP. 38.5° R.; PULSE 111; RESP. 16, AND ON THE WHOLE BODY AN ERUPTION OF BRIGHT RED ITCHING SPOTS, FROM THE SIZE OF A LENTIL TO THAT OF A SILVER SIXPENCE, SOME OF THEM SINGLE, OTHERS COLLECTED IN GROUPS. THEY WERE MOST ABUNDANT ABOVE THE RIGHT ELBOW, THE BACK, AND THE GENITALS; LESS ABUNDANT ON THE NECK AND FACE. THEY MAINLY PRESENTED A CENTRAL VESICLE, FILLED WITH SLIGHTLY TURBID, AND SOMETIMES PURULENT FLUID. THE SUBCUTANEOUS TISSUE WAS NOT SWOLLEN. THE ABDOMEN WAS SOFT, THE SPLEEN ENLARGED, THE OTHER ORGANS NATURAL. ON THE FOLLOWING DAYS THE REDNESS DIMINISHED, BUT THE EXUDATION INCREASED, SO THAT THE WHOLE AREA OF THE LARGER SPOTS BECAME COVERED BY VESICLES, AND THE SINGLE VESICLES WERE ALMOST WITHOUT HYPERÆMIA. THE VESICLES WERE USUALLY OF THE SIZE OF A PIN'S HEAD, FULL OF TURBID FLUID, AND IRREGULARLY DISTRIBUTED; IN SOME PLACES, AND ESPECIALLY ON THE BACK, IN INTERLACING CIRCLES LIKE SYPHILITIC LICHEN. THEY DRIED UP SPEEDILY, AND WERE REPLACED BY OTHERS, UNTIL ABOUT THE TWELFTH DAY, BY WHICH TIME ONLY BLuish RED SPOTS REMAINED, NOT LOSING THEIR COLOUR BY PRESSURE WITH THE FINGER. ON THE FOURTEENTH DAY APPEARED A NEW ERUPTION, NOT SO ABUNDANT, AND WITH NO VESICLES, BUT RESEMBLING ROSEOLA. IN TWO DAYS THIS BECAME PALE AND DISAPPEARED. BETWEEN THE SEVENTH AND THIRTEENTH DAY, THE MORNING TEMPERATURE STEADILY DECLINED FROM 32° TO 30°, WHILE THE EVENING TEMPERATURE WAS ALWAYS ABOVE 32°. FROM THE FOURTEENTH DAY THE MORNING TEMPERATURE WAS NORMAL, AND THE EVENING TEMPERATURE STILL 31° OR MORE. FROM THE SEVENTEENTH THE TEMPERATURE WAS NORMAL, BOTH MORNING AND EVENING. THE PULSE VARIED BETWEEN 106 AND 103 ON THE SEVENTH AND EIGHTH DAYS, BETWEEN 103 AND 92, UNTIL THE THIRTEENTH DAY, AND THEN SANK TO ITS PROPER STANDARD. HEADACHE AND VERTIGO, AS WELL AS PAINS IN THE EXTREMITIES, WERE ONLY PRESENT DURING THE FIRST FEW DAYS, AND, BY THE TENTH DAY, HAD WHOLLY DISAPPEARED. THE SPLEEN INCREASED IN SIZE, ON THE SEVENTH AND EIGHTH DAYS, TO BE 10 CENTIMETRES IN HEIGHT, AND 16 IN BREADTH; AND FROM THEN DIMINISHED, UNTIL, AT THE DISCHARGE OF THE PATIENT ON THE TWENTY-FOURTH DAY, IT WAS ONLY 5.5 CM. IN HEIGHT AND 9 IN BREADTH. THERE WAS SLIGHT OCCASIONAL COUGH AND PALPITATION, AND MUCH DEPOSIT OF LITHATES FROM THE URINE, BUT WITH NO ALBUMEN, AND NO DIMINUTION OF CHLORIDES. THE WEIGHT OF THE BODY DIMINISHED DURING THE FIRST FIFTEEN DAYS, AND WAS PARTIALLY REGAINED DURING THE FOLLOWING EIGHT DAYS.

Four other cases, agreeing in all essential particulars with the above, occurred in the early part of 1863; and it is assumed that the whole five may be taken to represent a special type of disease, which much resembles, and yet essentially differs from, ordinary enteric typhus. The cases also resemble acute pemphigus—the *febris vesiculosa* of Bärensprung—and, in some respects, herpes phlyctenodes; but they yet present many special characteristics of their own.

ART. 31.—*On the Treatment of Malarious Fever.*

By MR. HARE, Deputy Inspector of Hospitals, Bengal.

(Medical Times and Gazette, vol. xvii. 1865.)

The treatment of malarious fever in India has been revolutionized during the last ten years, mainly through the efforts of Mr. Hare (*vide* "Abstract," vol. xx. p. 253); and the long and elaborate essay from which the following quotations are taken contains the account of Mr. Hare's latest views and practice in this matter:—

"I have treated 6982 fever patients, with only one death in every 211½ cases, and I have to describe but one change in my treatment, which, however, I consider a most valuable and important improvement. I have, in fact, gone backward, it may be thought by some, in my antispasmodic treatment, and resort now, I am certain in some cases with great advantage, to mercury.

"I have not, however, lost any of my old confidence in quinine. I maintain still that given early and in sufficient doses, it is a perfect antidote; and I would cheerfully go back again to Calcutta and try my experiment again with quinine *only*, and a syringe to wash out the bowels with plain water. With water to wash out the bowels and apply to the head, and plenty of quinine, I want no other medicine for the treatment of the worst forms of tropical fever. Leeches, bleeding, purgatives, emetics, are, if not useless, at any rate unnecessary. You can cure all your patients without one of them. Why, then, do I now use calomel? I will explain.

"Quinine, as is shown above, when given in the large doses which are occasionally necessary to save life, in cases where the fever has been of some days' continuance, produces very dangerous and distressing symptoms; though I assert again that I have never yet had a case where the results were fatal, nor do I know any case of mine in which even the patient complained of any lasting bad result from it, and in a regiment I was with so many years I must have known these results, if there had been any. Yet I can easily conceive of such accidents occurring, where very large doses, as ʒi., were given at once, but with only one ʒss. dose to commence with, and ʒj. doses given at intervals of never less than three hours (and I have rarely, indeed, found more necessary), the symptoms of cinchonism will soon warn you to stop the medicine before sufficient can have been given to poison.

"However, besides this, any one who treats many bad forms of fever will often find himself puzzled how to act when the patient, as he often will, vomits his medicine. It may be written down, as an axiom in all cases true, give plenty of quinine to a case of malarious fever and it will be cured. But this is not always so easy in practice as it appears on paper; for if you cannot give the quinine because the patient vomits it, what are you to do? You are reduced to the same perplexity as Dr. Johnson was in his first trial of bark. With the very bad and long-neglected cases in the non-military General Hospital, this was the bane of all my pleasure; for I really would

have enjoyed my practice there but for the anxiety these vomiting patients sometimes caused me. When I went into the ward and asked how Smith or Jones was, the apothecary would tell me he was very bad, and had brought up all his medicine. There was no time for delay. The patient, if he could not retain the quinine, would probably die in the night. And I would go to his bedside, and persuade him, perhaps with difficulty, to swallow another dose; but before I left the ward, to my disgust, I heard the sound of vomiting again, till sometimes I was much perplexed. However, by not allowing the patient to drink anything, and by giving the quinine in pills, in port wine, in soup, or, if the patient were a spirit drinker, in brandy, I always contrived to give him sufficient to check the fever, and then his stomach would bear more. But still it was only by very careful management that I did so; and always with great discomfort and distress to the patient. In the General Hospital I had no choice. I was obliged there to continue the simple quinine or give up my experiment; but since then, while I was in Burmah with my regiment, and was trying there calomel and quinine in large doses in the treatment of malarious dysentery, I soon became convinced how much one drug assists the other in producing its peculiar constitutional effects. There are many cases of fever which I described above which cannot be salivated with the largest doses of calomel aided by mercurial frictions; but if you add quinine to these doses, you will find that salivation quickly appears.

"Dr. Morehead notices the same fact, p. 205. He says:—'I can state from my own observation that it is by no means an unusual occurrence in the course of remittent fevers, which in their early stages have required the exhibition of several full doses of calomel, to observe, after the recurrence of the fever has been prevented by quinine, a slight mercurial action to appear on the second and third days, though not more than a few grains of calomel or blue pill, in combination with quinine, had been given during those days.' Dr. Watson also, and many other writers on the treatment of intermittent fever, notice how much better and more quickly quinine acts the next day if a full dose of calomel be given over night.

"Again, when I left my Hospital, and had to treat children for fever, I found the little things sometimes obstinately refuse to take large doses of the bitter medicine, and often if they did take it, it was vomited; and I confess that I rarely now treat a young child for bad fever without mixing calomel with the quinine. Its effects on them, without producing any visible constitutional effects, are marvellous, and the quantity of quinine you are obliged to give is greatly reduced. My rule, therefore, now is, if a patient can take quinine in the quantity I think necessary for his safety, I give it only; but if he vomits it often, I give him a full dose or two of calomel, and I prefer the old ℞j. dose to any other from its inimitable effects in checking vomiting. I never have had occasion to exceed two doses; and I find I can then continue the quinine without any trouble to myself or patient. The quinine also need not be stopped, but given with the calomel; the calomel put dry on the tongue, and the quinine given in pills.

"To a child I give one large dose of quinine to start with, and try and repeat it; but if the little thing is sick and refuses it, I drop two or three grains of calomel dry on its tongue, and mixing three to five grains of quinine in jelly, I make it into a pill, and put it on the end of my finger into the child's throat. This cannot so easily be done with ten grains, which without the calomel would be required. Very soon, however, you will find that this treatment will check the fever; and you can then stop the calomel, and continue only the quinine. There is no necessity to salivate any one, or give a grain of calomel more than just sufficient to assist the quinine; and in many cases it will be found a great comfort to both patient and physician to give it in this way. I believe myself that as quinine is an antidote so is calomel, and that the two in combination have a more powerful and immediate effect than separately. Were we to be so unfortunate as to lose cinchona, which to us residents in the tropics would be as sad a loss as the fire which Prometheus restored, we must resort, I think, to the calomel system again; for bleeding alone and purgatives will not check the paroxysms of remittent fever. It requires, as Johnson soon found, something more to propel the stagnant congestion, and the salivation system only began to yield as quinine was gradually introduced. In the chronic forms of malarious disease, where we have time to produce the constitutional effects of arsenic by small doses, we could use it; for the results of arsenic are often in ague far more permanent than quinine.

"In conclusion, also, I must, in self-defence, explain other details in my practice. My published treatment may induce my readers to suppose that I treat every case of fever with ʒss. and ʒj. doses of quinine, and push it to the utmost; but it must be recollected that my published cases, and the treatment I have recommended, is only for *extreme* cases, and where there is urgent danger of life. Also I must be pardoned, in performing an experiment on quinine in the General Hospital, for pushing my treatment to extremes. Half the value of the results of my experiment would have been lost, if I had not done so, for it is only the extremes of any practice which show its really hurtful tendencies. And I confess that I might have saved much of the quinine which I gave to my patients during my trial; but I might have lost some of them had I tried to adopt this economy. I never witnessed any harm from giving over-doses; and I thought it right, to settle finally the disputes about the danger of quinine in head symptoms and local congestions, to push it to extremes; and no one, I think, can be sorry that I did so—not even my patients; but I do not do so now. I do not, moreover, push quinine through the paroxysm, if I have time, and can safely avoid it; and prefer the sweating stage to any other for my largest dose, for the febrile disturbance of the accession is unfavourable for the absorption of all medicines—quinine, calomel, and purgatives included. All I say is, if there is danger, do so. Only a few cases also require heroic doses, but these being more particularly described, assume the most prominent position in every description of systems of treatment, and are too apt to be considered as resorted to in

every case. I frequently give fever cases only ten-grain doses, oftener still only five grains three or four times a-day, and to a few none at all, but treat them with a purgative, low diet, and rest in bed. In justice, however, to your patients, you ought to give them a little quinine, for why let them be sick, when they may be well so much sooner? But when I see a patient with dull eye, cold, clammy skin, tremulous tongue and oppressed pulse; or another, with acute head symptoms, delirium, and impending coma; or a child, with spasmodic contraction of the thumbs, squinting, or actual convulsions, in a paroxysm, I do not lose time by preparing such cases for quinine with purgatives, &c. I know the danger impending, and I give them (at least the adults) ʒss of quinine at once, and follow it up with ʒj doses every three or four hours, according to the symptoms it produces, till they are fairly cinchonized, and if they vomit the quinine I add ʒj or ʒij of calomel. I never give more than ʒss of quinine in one dose, and prefer frequently repeated ʒj doses, because you can watch better the symptoms it produces, and stop it before the cinchonism becomes extreme.

"Also, with respect to diet, I do not offer or prescribe food for a young soldier just attacked with fever; I would give him little or nothing for the first day or two; but if I have a patient sent me who has had fever for a week or more, I feel sure that he is starving, although he may have no appetite—perhaps, even, he may loathe food, and I persistently make him swallow small quantities of soup, arrowroot, and milk, and if I know him to have been a spirit drinker, or even a free liver, I give him wine or spirit. This is all that I wish to impress on my readers: that malarious fever is not an inflammatory disease, but the direct contrary, and requires no depletion by bleeding or purgatives, and that the congestion of the liver, &c., which appear in fever, and the morbid secretions thence arising, are best cured by early and large doses of quinine, under the use of which the congestions disperse, and with them the morbid secretions. Also that we can support the strength of the patient without danger by proper food and nourishment. Let a patient be therefore the first day or two of his fever with but little food, but afterwards give it to him in moderation, and especially do not starve children."

ART. 32.—*On the Treatment of Malarious Fever by the Subcutaneous Injection of Quinine.*

By Mr. MOORE, of the Bombay Medical Service.

(*Indian Annals of Medical Science*, April, 1864.)

Mr. Moore states that he has lately employed, with great success, the hypodermic method of administering quinine in the treatment of malarious fever.

"I use," he says, "the strongest solution of quinine which can be prepared, viz., thirty grains of quinine, ten or twelve drops of sulphuric acid, and half an ounce of water. Of this, previously strained, I inject from half a drachm to a drachm, the former quantity con-

taining somewhat less than four grains of the active agent. With the exception of a little sulphate of soda, if the bowels are confined, I have used no other remedies in complicated cases of any type of malarious fever. When the spleen is enlarged, or a leucocythemic condition manifest, I prescribe, as an additional curative agent, one or other of the preparations of iron.

"I generally inject beneath the skin of the outer belly of the triceps, and sometimes over the deltoid. The operation, however, is equally effective on the thigh or calf; and in cases of large spleen, the action of the remedy appears intensified by injection over that organ. I use a small glass syringe furnished with a silver point, and introduce the latter beneath the integument for half an inch or less. The pain is not more than the prick of a needle, and indeed is often less objected to than the bitter taste of quinine. I have never observed the slightest irritation following the operation, excepting when performed with a small trocar and common glass syringe; and when quinine has been used in *suspension*, and not in *solution*. I therefore insist on the use of a proper instrument, and on perfect solution of the alkaloid. If the quinine is not invisible, the preparation is unfit for use; the syringe becomes clogged, and the areolar tissue does not appear able to absorb the solid material, which, hence, creates irritation.

"The best time to inject is shortly before the expected cold fit; but it may be done during the first stage with the effect of lessening, and occasionally stopping the paroxysm. When an accession is expected during the day, injection in the morning will, almost invariably, prevent the attack.

"In cases of remittent I have endeavoured to inject during the remission; but do not wait for this period. In severe cases, the injection should be repeated at intervals of four, six, or eight hours.

"I believe four or five grains of quinine, injected beneath the integument, are equal in their effects to five or six times that amount taken into the stomach; also that the results are more certain, and that relapsing attacks will be found to be less common; while the economy of the treatment is self-evident.

"I have now injected a considerable number of cases in the European General Hospital, Bombay, and elsewhere, and find the number of those who lose their fever after the first injection is upwards of 60 per cent. of all classes of intermittents. Some cases, however, require two, three, or four injections, and remittent attacks a larger number."

ART. 33.—*On the Subcutaneous Injection of Quinine for the Cure of Ague and other Marsh Fevers.*

By Mr. P. H. DESVIGNES.

(Proceedings of the Royal Medico-Chirurgical Society, Jan. 10, 1865.)

The author has had large opportunities of testing the value of this remedy in the intermittent fevers which were so common in

the district of Tuscany called the "Maremma." The use of quinine and arsenic, in the usual manner, having repeatedly failed, he resolved to try the subcutaneous injection of solutions of quinine. The solution he employed was a grain and a half in fifteen drops of water, acidulated with a drop of dilute nitric acid. With this he says that he has cured several hundred cases.

(c) CONCERNING CHRONIC DISEASES.

ART. 34.—*On the Different Modes by which Constitutional Syphilis may be Communicated.*

By Mr. HENRY LEE, Surgeon to the Lock Hospital, &c.

(*Lancet*, December 10, 1864.)

Until within a comparatively short period, it was a well-nigh universally received opinion in Europe, that syphilis could only be communicated by a primary syphilitic sore; that a chancre which was followed by secondary symptoms was always the result of contact with the secretion of a similar affection. Great and grievous have been the injuries inflicted by this theory both on single and on married persons. For anyone who in any way had contracted syphilis under the reign of this cruel dogma, was pointed at with the finger of scorn, was accused of having necessarily contracted the disease in some illicit way, and was not unfrequently most unjustly excluded from society. More than this: cases not unfrequently occurred, and have occurred under the author's own observation, where legal proceedings have been threatened or actually carried into execution, in consequence of a person having, as was supposed, contracted fresh disease after marriage, the parties instituting the proceedings being misled by the doctrine which then prevailed. The following case is given in illustration:—A gentleman who had had syphilis married. A few months afterwards his wife was found to have some indurated tubercles about the labia, with corresponding enlargement of the inguinal glands. A secondary eruption followed. The friends of the lady ascertained the nature of the disease, and being instructed according to the prevailing theory, accused the husband of having been unfaithful after his marriage. In this stage of the case the question was put to the author, "Can the husband have given the disease to his wife without himself having had a primary chancre?" Mr. Lee was enabled here entirely to clear the husband's character as far as his conduct subsequent to his marriage was concerned.

Nor is the evil which this false theory has produced restricted to private life. Many and various have been the plans suggested for the prevention of the extension of disease amongst our soldiers and sailors; but while much pains have been at times bestowed to prevent the contagion of primary disease, comparatively little attention has been given to the subject of the equally contagious nature of certain secondary affections. It is to be hoped that the Government

Commission appointed to investigate this subject will now take into its consideration the whole of the facts calculated to illustrate the real nature of the disease, the best mode of preventing its extension, and the most approved methods of treatment.

Scarcely six years have elapsed since a most eloquent and able course of lectures was delivered before the Medical Society of London, the principal aim and object of which was to prove that the whole series of secondary syphilitic affections were not inoculable, and most carefully and fully did the talented author of those lectures express the opinions which were then generally received. Since that time the subject has not, Mr. Lee believes, been brought fully under the notice of this Society, and he had ventured, therefore, to do so upon the present occasion.

Cases are then given illustrating the communication of the disease to a healthy woman from a diseased child by means of an indurated ulcer on the breast, and the communication of the disease by vaccino-syphilitic inoculation.

Numerous other cases of vaccino-syphilitic inoculation have now been recorded; and Dr. Viennois quite recently communicated to the medical congress at Lyons two fresh observations in addition to those which had previously been collected. On the 15th of May, 1852, six children were vaccinated. Five out of the six between three and four weeks afterwards had indurated ulcers on the inoculated parts. Each one of these five children communicated the disease to members of its own family, so that twenty-three persons were infected from this single source. The mothers and nurses of these children had first a chancre on the breast. The fathers of the children and the fathers of the nurses remained healthy until some form of secondary disease appeared on the private parts of the mothers and nurses, and then the husbands in some cases contracted the disease, which appeared in the form of a primary chancre. In a second case mentioned by Dr. Viennois, two children were vaccinated from the same source, and at the expiration of thirty-five days they both had an indurated chancre upon the inoculated parts.

Dr. Viennois has proposed, in order to prevent the recurrence of such serious mischief, that the inoculation of the cow-pox or the horse-pox from the animals themselves should be substituted for the ordinary mode of vaccination; and it appeared that at Naples there actually exists an establishment for the purpose of carrying out this idea. The cow-pox is there inoculated on heifers. These heifers are driven about to different parts of the town as donkeys are here for the purpose of furnishing donkey's milk. When anyone wishes his children to be vaccinated, a heifer is brought to the door; the driver removes a vesicle with a pair of scissors and gives it to the doctor on a pair of forceps, who then vaccinates the children with its contents. The fee for this kind of vaccination is five francs. The author observes that in speaking of vaccination our foreign neighbours very frequently use the term "vaccine pustule," and he has reason to believe that want of accuracy here might lead to some very serious mistakes. The vaccine poison produces a disease which essentially is not pustular. The vaccine lymph, examined under the microscope, contains no pus-globules at the time

when it is fit for use. If the vesicle were allowed to remain, no doubt pus-globules would be formed; and if the purulent fluid were inoculated, a suppurating sore might be produced instead of the proper vaccine vesicle. Attention to the nature of the inoculated fluid he (Mr. Lee) believes to be very important, and thought that many of the "bad arms" seen after vaccination might depend upon other matters besides the pure lymph having been inoculated. There could be no doubt, he remarked, that the action which produced a pustule was one of an entirely different nature to that which produced a vesicle; and unless these two actions were practically distinguished, medical men ran a great risk of inoculating a different disease to that which was intended. In these observations he did not allude to the presence of any separate specific poison in the inoculated matter, but simply to the kind of action which was intended to be communicated by the operation. If pus were inoculated, a pustule would probably result; it would be a suppurative action, and liable to all the complications which occasionally follow suppuration in a part. The author was informed by Mr. Tomkins, of the National Vaccine Establishment, that he had seen very serious constitutional effects follow the inoculation of the cow-pox taken from the cow. How far such effects depend upon pus being inoculated instead of lymph demanded our serious consideration. In the interest of science, we were called upon carefully to distinguish these two actions, and in practice certainly vaccination should never be performed with matter taken from a pustule.

In the year 1854 the author published some lectures on syphilis which had been delivered the previous year at the Lock Hospital; and, in the fourth lecture, he dwelt upon the two kinds of results produced by syphilitic inoculation. He then stated:—"In one case the appearances are in their origin—those of the adhesive inflammation; in the other, of the suppurative inflammation. And these constitute two very different classes of disease, both in respect to the local affections and their constitutional consequences." At this time every syphilitic inoculation was generally supposed to produce a "pustule;" and the production of this pustule was not only taken as the sure evidence of the syphilitic nature of the disease, but as a test for the necessity of giving mercury for its cure. Had we earlier learnt to distinguish the different kinds of morbid action which syphilitic inoculation gives rise to, much of the confusion which has reigned with regard to this branch of knowledge would have been avoided. It is now generally admitted that real syphilitic inoculation does not produce a pustule; that the disease which infects a patient's constitution commences as an abrasion, a pimple, or a tubercle; that the disease which does produce a pustule when inoculated is a disease of an entirely different kind, and that its effects are entirely local. Those who, therefore, maintained that the production of a pustule by inoculation was an indication for the administration of mercury, were advocating exactly the reverse of that which more recent observation has established.

There can be no doubt but that the real infecting form of syphilis, commencing with a form of adhesive action, will, during some period

of the existence of the primary affection, give rise to a puriform secretion, just as the vaccine disease will, after the ninth or tenth day, produce some pus-globules; but that does not in any way prevent us from recognising the essential characters of the disease, in its origin and in the induration which is subsequently produced, as quite distinct in one case as in the other from any purely suppurative action. Mr. Lee was led to dwell somewhat upon this subject, as the distinction which he drew between the two kinds of syphilitic sores as far back as 1854 has been called in question by some of our continental brethren. They have said that a true chancre will secrete pus; a fact which Mr. Lee had always allowed. And so, he observed, will a vaccine vesicle, especially if irritated.

In one case as in the other the disease begins without suppuration; and, if accidental causes of irritation be prevented, a very few days will leave the disease marked by the circular induration, and unaccompanied by any suppurative action whatever. One of the author's critics had supposed that in some drawings published last year of the microscopical appearances of the secretions of the two different kinds of syphilitic sores Mr. Lee had fallen into a mistake from not knowing the appearance of a pus-cell under the microscope. Mr. Lee thought he might at least have escaped this criticism had he mentioned that his friend, Prof. Beale, had been good enough to make the drawings in question for him.

The two kinds of action which were now described were very important, as enabling us to understand some forms of inoculation which otherwise would appear to follow no fixed law; particularly cases in which there has been a twofold inoculation, and where a mixed form of chancre has resulted. Such cases are not unfrequently observed in practice; but the evidence of their twofold origin is there not so satisfactory as if it had been the result of experiment. The evidence from direct inoculation is not, however, wanting. M. Melchior Robert has shown that if the secretions from the two kinds of chancres be inoculated by a single puncture on a healthy person, a soft chancre originating in a pustule will first appear, and subsequently an indurated chancre followed by secondary symptoms. M. Lindwurm has proved that if the secretion from an indurated sore be inoculated on a soft or suppurating sore, that sore will gradually be transformed into an indurated chancre, which will be followed by secondary symptoms; and M. Rollet has often demonstrated the fact that if the pus from a single chancre be placed on the surface of an indurated sore, the affection will assume all the characters of the "mixed chancre."

The limits of the present communication do not allow the author to go more fully into the subjects of the different kinds of secondary affections which are communicated by inoculation, nor of the inoculability of different kinds of secretion, as the milk, and the semen derived from a syphilitic subject. He only remarks, that, as far as his own knowledge went, he is not prepared to admit the inoculability of any secretion derived from a syphilitic subject without some increased or diseased action in the organ producing that secretion. Thus, the saliva from a syphilitic person with

a healthy mouth would not, so far as he knew, produce syphilis in another person. The milk from a healthy breast of a syphilitic woman would not produce the disease in the mouth of a child. The semen from a syphilitic person with healthy organs of generation would not produce syphilis by being brought into contact with the mucous membrane of the vagina. But let any of the parts become diseased, or let them be subject to increased action in a syphilitic person, and then the secretions from those organs have a power which they had not before, and might communicate the disease directly to any part with which they came in contact. The following case is given in illustration :—

A gentleman requested to know what was the matter with him, and was informed that he had a chancre which would infect his constitution. He said it was impossible, as he had never seen but one person, whom he had known for a long time, who had nothing the matter with her, and who was quite willing to be examined if the fact were doubted. This person was accordingly examined. The mucous membrane of the vagina was red and congested, and a muco-purulent discharge issued from the vagina. There was no sore or abrasion of any part. On examining the chest and arms a distinct syphilitic eruption was visible, which she said she had had for some years, and believed to be the "nettle-rash." It was then apparent in this case that the man had not been infected by this person, although in the way of becoming so, until, from some cause or other, some local increased or diseased action was produced in the organs of generation, and that then the secretion from those parts was inoculable. In this case the patient obtained the certificates of two medical men to say that she was free from disease.

ART. 35.—*On the Transmission of Syphilis by Vaccination.*

By Mr. —

(*Medical Times and Gazette*, January 21, 1865.)

A discussion has arisen at the French Academy of Medicine having for its object to consider whether a report of a most mischievous character should be presented to the Government on the part of this learned body. To the Academy is intrusted the duty of performing gratuitous vaccination in Paris, and encouraging by rewards its practice throughout the country, and it makes an annual report to the Minister as to the manner in which the duty is performed. This report is prepared by the Director of Vaccination, and is usually agreed to as a matter of course. This year, M. Depaul, the present Director, appended to the usual administrative report what he termed a scientific portion, in which he represents to the Minister that the transmission of syphilis by vaccination is a well-ascertained fact, calling for special precautions. When the report was read to the Academy, the usual formal sanction was at once, and most properly, denied to this portion of it; and its future consideration and discussion, under the title of "Project of a Report," was deter-

mined upon. In his projected report, M. Depaul adduces no new cases in proof of his statement, but merely refers to those which have been before the public during the last few years; and he labours with all the ingenuity of a special pleader to reconcile the numerous discrepancies there present, and to apologize for the shortcomings of the corroborative evidence. He also makes a somewhat severe attack on M. Ricord for the obstinacy with which he so long adhered to the Hunterian doctrines, this being, in the reporter's opinion, a chief cause why the reality of "vaccinal syphilis" has not long since been admitted. After all, M. Depaul admits, even on his own view of the case, and with all the extreme latitude he exhibits in his reception of ill-authenticated facts, that the occurrence of "vaccinal syphilis" is excessively rare, only some thirty or forty cases being recorded, notwithstanding the millions of vaccinations which are constantly taking place; nor is he aware, amidst the two or three thousand annual vaccinations performed by the Academy, that such an occurrence has ever been suspected.

ART. 36.—*On Syphilis.*

By Dr. BOLZE.

(*Prag. Med. Wochenschr.* 38 and 39, 1864.)

That many of the difficulties in the way of understanding and arranging syphilitic phenomena may be overcome, has been first shown by the recent discovery that their differences to superficial examination are only apparent, depending upon differences of anatomical structure in the organs assailed, and that they are identical in histological characters.

Until very lately, surgeons have not been able to pronounce positively whether certain morbid changes were specific or not; and Virchow, in 1858, said that degenerations, the result of syphilis, presented no determinate characters. The recent inquiries of Wagner, however, encourage hope that the time is not far distant at which we shall be able to pronounce certainly, by microscopic examination alone, whether given changes are dependent upon syphilis or not.

1 An examination of the condition of the several bodily organs leads to the belief that the syphilitic disease is the sum of their affections; and that what we are accustomed to call syphilitic dyscrasia results from the disturbances of function that these affections entail.

The surmise of Ricord, that the presence of the specific virus in the blood was the cause of syphilitic marasmus must be laid aside as untenable; since, if it were well founded, as the blood is always most highly charged with virus at the beginning of the disease, the marasmus would be among the earliest phenomena observed, instead of occurring, as it does, only at a late period. Syphilitic chlorosis, moreover, does not depend upon the spreading of disease among the lymphatic glands, but rather upon the gradually increasing

abnormalities that interfere with the discharge of various important bodily functions.

The researches of Virchow and Wagner have given the *coup de grace* to the doctrines of Hermann. These observers have proved microscopically, apart from all other evidence, that the anatomical changes produced by mercury are not identical with those produced by syphilis.

Histology promises further to solve the questions which so much occupy those who differ with regard to the existence of one, or of two, syphilitic poisons. The characteristic deposition of cells must commence with the first stage of diffuse infiltration, and would enable us to decide, before it had proceeded to any great extent, or to the formation of nodules—that is to say, before what we call induration had occurred—whether syphilis was to be expected or not.

Moreover, the conception of latency receives a natural and sufficient explanation in affections of internal organs, in which the syphilitic nodules—*e.g.*, in the lungs or liver—require considerable time for their development before they do sufficient mischief to interfere with the functions of the parts concerned, or to provide germs for their own reproduction in other organs.

Surgeons have been led by observation to conclude that the appearance of a nodule at the place of infection affords the first visible indication that infection has occurred; but this visible indication must be preceded by a stage of incubation, during which the processes occur which promote dissemination. In this period only cell changes occur. The sources of the extension of the disease must be sought either in the cells or the fluids, but researches into the movements of cells do not support the former supposition; since it is proved that infection may occur without any visible local affection. This part of the question requires the further attention of histologists.

While it remains undetermined, we must apply ourselves to those changes which belong to the second period of the disease. Among these we find those histologico-anatomical changes which lead to a pregnant and characteristic arrangement of the near and remote consequences.

The first appearance which the microscope discovers in an organ attacked by syphilis is an enormous new formation of cells, which are either deposited in the tissues as a diffuse infiltration, or are aggregated into nodules ranging from the size of a millet seed to that of a fist. In the living subject the diffuse infiltration is not discernible, even when it affects visible mucous membrane or skin, and the nodules are, therefore, the first phenomena that can be recognised by the surgeon. Why diffuse infiltration should occur, in some cases, without nodules, in others nodules alone, and in others, both forms of disease together, has at present not been explained.

Next are found the ordinary local forms of syphilis: that is to say, ulcers with excavation, cicatrices, simple drying, and fatty atrophy, with which changes the cycle of local affections is complete. Morphologically regarded, these changes are all products of

mortification, since in them all cellular structure is disorganized; and they are differently viewed by the histologist and the surgeon: the latter having learnt to consider them as signs of that which, for want of a better expression, has been called the syphilitic virus.

Since histologists can only examine preparations, there are two phases of syphilis of which their knowledge is less exact. These are, first, an arrest of the morbid processes, followed by resorption without residual changes of texture; and, secondly, the separation of mortified tissue. The latter occurrence, which the author has observed in the tonsils, gave occasion to his paper.

All the characteristic microscopic appearances of syphilis are found in the tonsils; although the syphilitic diseases of these organs are copiously included in treatises, under the collective word *angina*.

The first outbreak of syphilitic disease of the skin, is commonly accompanied by an affection of the mucous membrane of the throat and velum, which, extending to the arch of the palate, has been observed under the name of *angina*, and which, as it only attacks the mucous membrane, may be distinguished as *catarrhal*. It is extremely rare to see a papular infiltration attending the close of this condition. The disorder frequently returns, and accompanies those febrile attacks to which syphilitic patients are so prone.

The case is different at later periods, after disease of the bones has shown itself. We may then often see the syphilitic affection limited to the arch of the palate and the surfaces of the tonsils, the throat and velum not being involved. In such cases it is also possible to distinguish the disorder of the mucous membrane, and the sub-mucous tissue, from that of the substance of the tonsil within its fibrous sheath.

In disease of the mucous membrane the volume of the tonsil is not increased, the membrane is reddened and slightly swollen, and presents the appearance of a purplish-red surface covered with numerous yellow points. This condition passes away and leaves no trace. In other cases, after a period of more intense redness and greater swelling of the mucous membrane has subsided, there remains a deeply orange-tinged petal, which lasts for weeks, and only gradually subsides. In others again, the stage of redness is succeeded by a watery secretion, which leaves upon the mucous membrane a number of small, normally coloured depressions. Very rarely the tonsillar membrane remains for some time covered by a film of fatty lustre; and when this disappears, the surface is marked by a cicatricial network. Frequently the reddened membrane is covered by a whitish-grey coating, which is renewed when stripped off. Being unattended by pain, this condition may easily escape observation, until at last the mucous membrane peels off in shreds without leaving a scar. At other times the repeated removal of the coating will leave a white smooth cicatrix.

When the substance of the tonsil within its fibrous sheath is affected, whether the connecting tissue or the globules of the gland, the gland is raised forward out of its fold, without any preceding redness or swelling of the surface; although, after this projection

has continued for some time, the mucous membrane may at last become involved. The swelling commonly subsides entirely; but an excavated ulcer is frequently formed, which may exist for weeks without pain, and then undergo cicatrization. In this case, the tonsil will be small and withered, its remains forming a whitish body the size of a pea, visible between the pillars of the fauces.

In three cases the author has met with a result previously undescribed.

CASE 1.—A powerful man, in the prime of life, came to him in 1846, complaining of erratic pains. He had frequently had sores on his penis, the inguinal and cervical glands were enlarged, and the shoulders, forearms, chest, and back were studded with numerous brown-pigment spots. From the left tonsil there projected forwards a whitish-grey pulpy mass. On the patient's second visit, three days later, this degenerated tonsil had disappeared, and its place was vacant. The pains had disappeared. Of the previous history of the throat affected the patient knew nothing.

CASE 2.—A soldier, who had suffered from various affections of the genitals, applied in 1860. He had a chronic eruption, in the form of numerous ulcers on the back, chest, and thigh, with rhyades between the toes, and tubercles on the nates. The right tonsil was swollen, its surface of a greyish-white pulpy appearance, the fauces a little reddened. The patient was unconscious of throat disease, and swallowed or spoke without pain. The author saw him daily, and saw no change in the tonsil until the twelfth day, when the whole mass had disappeared, and the faucial groove was empty.

CASE 3.—In 1861, a strong girl, twenty-four years old, came to the author. She had several times been in hospital for syphilitic affections. At the lower part of one arm she had an oval ulcer, covered with yellow crusts, and as large as a crown piece. She had cicatrices on the vulva, uterine blenorrhoea, with erosion of the cervix, and several small painless glands in the inguinal and cervical regions. The right tonsil appeared enlarged, without redness of the mucous membrane. The speech was normal, the hearing unimpaired. During the subsequent days the tonsil visibly increased, so that at last it compressed the uvula. There gradually appeared upon the mucous membrane a dull white spot, which in time spread over the whole tonsil. The mucous membrane, still visible, displayed a fissure, through which a granular white mass protruded, that daily became more prominent, and at last resembled the buds of a cauliflower. The fissured membrane underwent gradual retraction, and portions of the projecting mass could be removed by a spatula. Such portions were a greyish-white, cheesy pulp, in which granules as large as groats were imbedded, and were free from fœtor. One morning the whole had disappeared, and the tonsillary space was empty. After a while, the anterior and posterior pillar fell together. In the winter of 1863, the patient came again under observation. The velum was drawn to the left, the right tonsillary space had disappeared, and its place was only marked by mere rudiments of the faucial arches.

As in all these cases the syphilis was of long standing, the author concludes that this form of affection belongs to the later periods of the disease.

The parts of the body which may be the subjects of a similar kind of morbid action cannot yet be enumerated; but it is probable that the bronchial tubes and the intestine are among them.

The sputa of patients who, after long-continued syphilis, die with symptoms of phthisis, have not been microscopically examined; but

the researches of Wagner render it almost certain that such cases are, in reality, not tuberculous, but syphilitic affections of the bronchial mucous membrane. The same probably holds good of fatal colliquative diarrhœa in old syphilitic cases. Lastly, syphilitic caries and necrosis may depend upon an essentially similar process.

ART. 37.—*Case of Supra-renal Melasma.*

By Mr. THOMAS HAYDEN, Physician to the Mater Misericordiæ Hospital, Dublin.

(*Dublin Quarterly Journal of Medical Science*, February, 1865.)

Mr. Hayden analyses the histories of thirty-seven cases of this disease, reported in the original memoir of Dr. Addison, and more recently in the writings of Drs. Wilks and Habershon, and his conclusions are very much the same as those which have been arrived at by others. His own case is as follows:—

CASE.—John Irvine, aged sixteen years, was admitted into the Mater Misericordiæ Hospital, under my care, July 16, 1864. Six months previously he was attacked, rather suddenly, with severe headache, which, with short intermissions, had continued to the date of his admittance into hospital. About the same time a large blotch, the size of a half-crown piece, and of a dark brown hue, appeared upon his forehead. He had suffered much from constipation, and about ten days before he came under my notice, having taken a dose of castor oil, he experienced for the first time severe pain in the abdomen. Three days subsequently, after a certain degree of languor and loss of appetite, a crop of yellowish-brown spots of minute size appeared over the entire cutaneous surface, but in greatest number on the abdomen; and the pain on the abdomen becoming more urgent, he sought admittance into the hospital. The face was of a dark olive tint; a large spot of deeper hue was observable in the forehead, near the roots of the hair; the conjunctivæ were of pearly whiteness; the surface of the body generally was similarly discoloured, but less deeply; and a number of minute spots of a deeper brownish tint were visible on the abdomen. The pulse quick and weak, the tongue dry, the bowels confined, the urine normal; he suffered occasionally from severe headache and pain in the hypogastrium, and on the slightest movement the sight became dim, and the respiration hurried; his weakness was such that he was unable to walk to his bed without assistance. The heart's action was feeble, but the sounds were normal. A minute physical examination of the chest and abdomen yielded no evidence whatever of organic disease.

July 27.—Suffered from sick stomach last night; slept very little; headache very severe; pulse 130, weak.

July 30.—Takes scarcely any food; pain in abdomen more severe; pulse 138.

August 3.—Stomach has been sick all night; took no food; complains of great thirst; pulse 150; and barely perceptible.

August 6.—Stomach very sick; extremities cold; pulse cannot be felt at the wrist; tongue dry.

August 7.—Has vomited everything he took during the night; has likewise had empty and bilious retching; pulse at the wrist not perceptible; counted by the heart it was 140; respirations 27; complains of urgent

thirst, which is relieved by a little ice. 9 P.M.—Heart's action almost imperceptible; respirations 40 in the minute; at 10, P.M., he sank from exhaustion, and without a struggle.

Post mortem examination six hours after death.—Body much wasted. Heart small and comparatively bloodless, valves healthy. Liver of average size and of dark olive tint. Spleen in all respects normal. Kidneys of average size and apparently healthy on section. Both supra-renal capsules very much enlarged, hard, and nodulated; the left much larger than the right; the former presented on section the appearance of pale cheese, and in the centre was found about a teaspoonful of thick white puriform matter; right supra-renal capsule not divided. The pale yellow deposit in left capsule examined microscopically was found to consist of small, granular, ill-defined cells, and the granular *debris* of similar cells, amongst which were dispersed molecules of a highly refractile, and apparently fatty character.

Whilst this patient was under treatment in hospital his urine was repeatedly examined, and always found normal in quantity, specific gravity, and reaction, devoid of albumen, and free from deposit of any kind, with the exception of a little mucus.

The treatment consisted mainly in various preparations of iron, sedatives, counter-irritants, and towards the close of the case, when the stomach had become intolerant of food of any kind, of nutritive and stimulant enemata.

ART. 38.—*On Leprosy in India.*

By Dr. H. V. CARTER, Assistant-Surgeon to the Jamsetjee Jejeebehoy Hospital, Calcutta.

(*Medical Times and Gazette*, and *Glasgow Medical Journal*, January, 1865.)

Attempts have lately been made, both in Germany and England, to obtain extensive and reliable information as to the pathological history, geographical distribution, and causation of leprosy (*Lepra Arabum, Elephantiasis Græcorum*). With this end in view, Professor R. Virchow, of Berlin, appealed in 1862 to physicians, historians, and travellers to afford him materials for composing a history of leprosy. A portion of the information he obtained has appeared in the eighteenth volume of Virchow's *Archives*. But, as he states, there are still important gaps to be filled up, even in the history of leprosy in Europe, and accordingly in April of last year he issued a fresh request for information on the subject. In his letter, dated 18th April, he states that he has obtained scarcely any details with regard to the state of lazarettos in Austria, and that next to nothing is known of the history of the disease in Westphalia, Hesse, Hanover, Oldenburg, Holstein, and Eastern Prussia. There is also almost an entire absence of information with regard to the Slavonic countries. If it be true, as Richter asserts, that leprosy first appeared in Russia in 1426, a period when it had assuredly begun to disappear in other European states, this fact would alone be a valuable addition to the current knowledge of its history. In August, 1862, at the request of the Duke of Newcastle, the Royal College of Physicians of London appointed a committee to collect information. The committee drew up a series of interroga-

tories, which were sent to the various medical authorities both in our Eastern and Western colonies. We have before us the first fruits of the harvest of information which may be expected, the replies furnished by assistant-surgeon Dr. H. V. Carter, of the Jemsetjee Jejeebhoy Hospital. Dr. Carter's contributions on this subject are especially valuable, as for several years he has paid considerable attention to it, and has had a wide and fruitful field for observation. Dr. Carter states that leprosy is well known in most, if not in all parts of India; that it prevails extensively in the Bombay presidency; and that it presents itself under three forms or varieties:—1. The first is an eruption on the skin, probably allied to *Lepra* (*Græcorum*), and accompanied by anæsthesia. This form has not been accurately distinguished by writers, but it is probably the *Leuke* of the Greeks, the *Baras* or *Beres* of the Arabs, and perhaps the *Barat Lebena* of the Hebrews. To it belongs the *White Leprosy*, or *Shvet-kusta* of India. 2. The second variety is characterized by "anæsthesia of the skin of the face, ears, and extremities; followed in the latter case by atrophy, interstitial absorption, and occasionally ulceration of the benumbed parts, notably of the fingers and toes." This is the most frequent form. It is the *Guleet-kusta* or *Sunbahiree* of the Hindoos, and has been described as anæsthetic leprosy or articular leprosy. 3. The third form is marked by tumefaction or tubercular thickening of the skin. It principally attacks the face and extremities, but is less marked on the trunk. This variety is best known and has attracted most attention in the West, but it is not the commonest in India. It is the *Tubercular Leprosy*, the *Elephantiasis*, *Leontiasis* of the Greeks, the *Lepra* of the translators of the Arabian writers, the *Jezam*, *Da-al-asad* (lion disease) of the Arabs, and the *Ructa-kusta*, *Ructa-pitia*, *Maha-viadhi* of Hindoos. Dr. Carter considers the second form to be the typical and most invariable, but he asserts that all three are but varieties of one common affection; they seldom occur separately, being almost always combined at certain stages; different members of the same family may be affected with each. They occur under the same circumstances and in the same locality; and a parent, the subject of one variety, may transmit another to his offspring. On the subject of premonitory constitutional symptoms ushering in the affection, Dr. Carter's experience is at variance with some other observers. He states, as the result of his inquiries, that there are no special or invariable premonitory phenomena. What the patient sees is generally his first intimation. The tubercular form appears soonest to lead to a fatal issue, evidencing perhaps a deeper taint than the more common or anæsthetic variety. The eruption alone does not seem materially to shorten life. Taking the disease as a whole, its duration may extend to twenty years, but from five to fifteen years is probably the usual period. In the town of Bombay the mortality seems to reach its maximum at about the age of thirty years. The male sex furnishes the most frequent cases of leprosy. The proportion of males to females is said to be about four to one. Leprosy principally attacks the native races. The mortuary returns of Bombay

for 1860 show that the greatest number of deaths from the disease had occurred among the native Christians; next among the Marathas or low-caste Hindoos, particularly the latter; then follow the Mussulmans, the Parsees, and the vegetable-eating Hindoos, whilst no case was furnished by the Jewish and European populations.

Although no caste is exempt, leprosy principally affects the lower orders of native Society; the greater number of lepers are from small hamlets and rural districts, but many are from the towns. These districts are principally on the sea-board, but the disease is also found inland, and Dr. Carter does not believe it to be essentially connected with sea-coast residence. Neither is it to be limited to low land, as it occurs in the high districts of the Deccan, but Dr. Carter thinks that it seldom arises *de novo* in cool and dry localities. With regard to the effects of hygiene and mode of living, it does not appear that those affected have, previous to the appearance of the disease, been under influences different from their neighbours. Dr. Carter thinks that leprosy is a cachexia, comparable in some respects to the syphilitic and strumous. Like the latter, it is clearly hereditary. There is no direct evidence that it is dependent on, or connected with syphilis; but Dr. Carter strongly suspects that the two diseases are essentially related. Leprosy, however, does not appear to be transmissible by sexual intercourse, and Dr. Carter knows of no reliable evidence which would prove it contagious. He is inclined to think that the disease may be on the decrease. He believes it to be a constitutional affection having a local manifestation, and no more spontaneously curable than syphilis or struma. With regard to the causes of the disease, he thinks they require to be investigated *de novo*, and suggests that a special commission be appointed to investigate them. Dr. Carter, from the data he has collected, places the following districts in order of furnishing the greatest number of cases:—1, The Concan generally; 2, Guzerat; 3, The Deccan and table-land; 4, Rajpootana; 5, Kattiawar; 6, Kutch; and 7, Scinde.

SECT. II.—SPECIAL QUESTIONS IN MEDICINE.

(A) CONCERNING THE NERVOUS SYSTEM.

ART. 39.—*Insanity versus War.*

By Dr. NICHOLS, Superintendent of the Government Hospital for the Insane in the United States.

(*Medico-Chir. Review*, April, 1865.)

Dr. Nichols reports in forcible and clear terms, as to the effects of the civil war in America upon the product of insanity. He says:—

“The department of the interior will learn with interest, we doubt not, that the number of the insane received into this hospital during the year under review was greater than the greatest number

ever received in the course of any one year by any other one institution on this continent; also, that owing to the immense armies and very large naval forces with which the war has been, and still is, prosecuted, and the specific sources from which our patients are mainly derived, a larger proportion of the cases received were affected with acute forms both of mental derangement and of idiopathic bodily disease than were ever before, in the course of one year, admitted into any one establishment on the globe.

"It should not be inferred that the war has been a prolific *moral* cause of insanity either among the men of the land and naval forces, waging hostilities against the common enemy, or among civilians of either sex or of any class. In not more than two per cent. of the four hundred and ninety-three cases received from the army since the war began has even the exciting cause of mental disorder appeared to have been either the profound excitements attending a personal participation in active military hostilities prosecuted on the largest scale, a sense of great personal danger in battle, or anxiety and misgivings respecting the result of a great contest, in which every man of much moral sensibility feels the deepest personal stake.

"The existence of more or less home-sickness among the national troops—perhaps the most pardonable weakness which a citizen soldier in the field can display—has been rendered evident by the character of the morbid manifestations exhibited by several of our army patients.

"Excepting a small proportion of cases caused by intemperance, cranial injuries, tumours, and other organic cerebral affections, necessarily sooner or later disturbing the mental manifestations, the insanity which occurs among the volunteer and other soldiers drawn from high temperate latitudes, campaigning in the lower latitudes of the same zone, appears to us to be, in most instances, one of the extreme consequences of a depression of the vital forces. The best constitutions are subject to such *sthenic* diseases as pneumonia and acute rheumatism, but with the exercise of a fair amount of prudence, they are often invigorated from the first by active service in the field. The weaker of the men, uninured to a soldier's life, are overmatched by the privations, exposures, and fatigues of active service; especially when serving in a malarial region, they first become thin and enfeebled, and then, upon some extraordinary exposure or fatigue in such a state of debility, there supervenes either an intestinal flux or a low form of fever, sometimes both, as distinct diseases. It is in the course or at the close of this series of agencies, which impair the strength and tone of the nervous system, that unsound mental manifestations begin to exhibit themselves.

"Most of the great political and social convulsions that have occurred in the course of the modern history of enlightened nations, have been attended with a manifest increase of insanity among the peoples most affected by such upheavings of society: but contrary to the anticipations which history authorized us to entertain at the outset of the struggle, the admission of civil cases into this hospital, situated in the very midst of the perturbations of the war, has been

fewer during the last two years than before. We find that this exceptional feature in the domestic strife of arms in which we are engaged is noticed by the medical directors of the two largest institutions in the loyal States.

"This important exception to the teachings of previous history cannot be accidental. It has been too uniform and too long continued for that to have been the case. It must be due to some peculiarities either in the character of our people, or in that of the war itself."

ART. 40.—*Chemosis of the Conjunctiva as a Symptom of Suppurating Meningitis.*

By Dr. LEYDEN.

(*Virchow's Archiv*, No. 1, vol. 29; and *Medical Times and Gazette*, August 13, 1864.)

Dr. Leyden wishes to draw the attention of the profession to this circumstance, as within a short time he has met with three cases in which chemosis of the conjunctiva constituted a very distinct phenomenon, all the other structures of the eye retaining their complete integrity.

ART. 41.—*A Case of Extensive Acute Myelitis, with Remarks.*

By Dr. RADCLIFFE, Physician to the Westminster Hospital, and to the National Hospital for Paralysis and Epilepsy.

(*Lancet*, December 3, 1864.)

The following case belongs to a class in which good clinical illustrations are much needed. It is one of very extensive acute myelitis—a malady which is certainly uncommon enough, and of which as certainly the symptomatology in some points requires careful revision. The notes which were taken at the time are these.

Charles K—, aged twenty-six, a draper's assistant, unmarried, admitted into the National Hospital for Paralysis and Epilepsy, Jan. 9th, 1864, under the care of Dr. Radcliffe.

Present State.—The patient says that he has lost all power of movement and feeling in the lower half of the body, that he cannot pass his urine, that on two or three occasions his stools have passed unconsciously, and that he has been in this state for six days. He complains of some pain in the left side of the chest, but not elsewhere. On examination, the paralysis and loss of sensation are found to be complete, and to extend as high as a line drawn horizontally around the body about four inches below the ensiform cartilage. No reflex movements or sensations are produced by tickling the soles of the feet. No pain is produced by percussion or by applying a sponge wrung out of hot water along the spine; but a sensation of heat is communicated to the patient by touching the surface for the space of an inch above the line which marks

the upper limits of the paralysed and anæsthetic parts. This sensation is felt all round the body, and not over the spinal cord exclusively or especially. The breathing is almost entirely diaphragmatic, the upper intercostal muscles acting a little, but not the lower. The breathing is hurried, the *alae nasi* work very much, and the lips are bluish. The air-passages are loaded with phlegm, especially on the left side, and there are frequent attempts to overcome this difficulty by feeble coughing. The skin is moist, and of about the natural temperature. The pulse is hurried, but not remarkably weak. The tongue is clean, the appetite is wanting, but there is no thirst. While the patient was turned on his side in order to allow his back to be examined, he passed a stool involuntarily, and it was plain that he had no knowledge of what had taken place until his nose took the alarm. At present the penis is flaccid; but it appears on inquiry that a state of unusual erection has been no unfrequent event since the commencement of the illness. The bladder is now distended with urine. The patient himself is of medium stature, light weight, good build, and bilious temperament. He seems hopeful and intelligent; and he talks readily and willingly, but in a very feeble and low voice.

Previous History.—On the night following Christmas-day last the patient was seized with a severe cold. He was hot and cold by turns; he had excruciating pain in the head, and constant and profuse running from the nose. These symptoms were so severe as to render it necessary to call in medical aid; but in the course of a few days they had all passed off. On the 2nd of January his appetite, strength, and spirits had fully returned. He felt quite well—"so well," he says, "that I could have jumped over the chairs." On the next day (Jan. 3rd) he went out between eleven A.M. and noon, and walked for three-quarters of an hour. He was well wrapped up. While out he felt quite well, and when he returned home he ate and relished a moderate dinner of fowl and potatoes. At this meal he drank neither malt liquor, nor wine or spirits. At four P.M. he had tea, and enjoyed it. Shortly afterwards he began to experience peculiar sensations in the feet, "lower joints," and buttocks—"an uneasy nervous twittering." A little later he became restless, and could not sit still in his chair. For a while he walked up and down the room without experiencing any difficulty in movement. At any rate he found it more easy to walk than to sit. When at last he attempted to sit down, the uneasiness and restlessness had increased to such a degree that he found it impossible to keep his seat. Then he decided on going to bed. He walked upstairs with little or no difficulty, and soon found, to his great surprise, that his urine did not flow at all freely. He also began at this time to experience some slight uneasiness at the end of the penis. After lying down this feeling of uneasiness increased greatly, and so also did the general uneasiness and restlessness. He says, "I tossed about like a maniac." A friend who is now at the bedside says,—"I saw him about eleven P.M. of the day on which he was attacked. I thought he was suffering from severe rheumatic pains in his legs, buttocks, and penis. For some hours these pains were excruciating. I had never before seen a human being suffer so much. He tossed about in dreadful agony. He roared out with pain, and when not roaring he groaned." After suffering in this manner for five or six hours, he fell asleep, and slept till breakfast time. Upon waking in the morning he found that he could not move his legs and hips, that he had altogether lost the power of emptying his bladder, and that he was without feeling in the lower half of the body. He also found himself freed from the miserable or painful feelings which had kept him in a state of perpetual unrest before he fell asleep. On asking him whether these feelings were of the character of severe pain, he

says, "No," "not exactly," "worse than pain," "a miserable feeling, which made it impossible to keep still;" so that the friend's account, which has just been quoted, does not quite tally in this respect with the statements of the patient himself.

It appears that the patient has always led a perfectly sober and well regulated life, and that he has never before suffered from any indisposition, with the exception of scarlet fever in childhood; nor does there appear to be anything wrong in the family history.

Mr. Thane, who attended the patient professionally before his admission into the hospital, says that he has been evidently losing ground from the beginning, but that his state now is substantially the same as that which existed at the beginning. He also says that catheterism was necessary from the first.

The treatment ordered was beef tea and brandy in liberal quantities (solid food was objected to), catheterism, and a draught every four hours containing four grains of iodide of potassium. This was at half-past four P.M.

Eleven P.M.—No material change. Surface moist and perspiring. Temperature of left leg, 84° Fahr.; of thorax, beneath the clothes, 84°. Penis in a state of strong erection. Urine: sp. gr. 1015, decidedly acid, and containing no sugar or albumen. The passage of the catheter into the bladder was in no degree perceptible to the patient. In the course of the evening the left eye was examined by Dr. Hughlings Jackson with the ophthalmoscope, without finding anything worthy of notice in the retinal vessels. The iris also was seen to be neither dilated nor contracted before the application of the atropine.

Jan. 10th.—Four A.M.: Somewhat less exhausted. No greater freedom in the breathing. No sleep. Catheterism. Urine: sp. gr. 1015; decidedly acid.

Ten A.M.—At seven A.M. there was a severe rigor, which commenced in the right arm, and then extended first to the back and afterwards to the whole body. This rigor continued a full quarter of an hour, and was followed by profuse perspiration. During its continuance the paralysed parts were very cold; after it had ceased, these parts became warm again, and as reaction took place the cough almost ceased, and so did the trouble of the breathing. He looked more comfortable, but the respiration still continued to be almost entirely diaphragmatic. Pulse 112. Catheterism. Ordered to have half an ounce of port wine every twenty minutes, and as much beef tea as he could be made to drink.

Half-past five P.M.—Pulse 140. Respiration 40, and now completely carried on by the diaphragm. The intelligence, as it would seem, is quite clear; the voice very low, and speaking only possible with great effort. No longer complains, as he did at first, of difficulty of breathing, except for a moment or two after waking from an occasional and very brief doze. The anaesthesia extending upwards. Superior extremities warm; inferior extremities warm and moist. The iodide of potassium to be discontinued, and to have instead, every four hours, this draught: Chlorate of potash, ten grains; chloric ether, twenty minims; decoction of cinchona, one ounce.

Eleven P.M.—Pulse 150; respiration 36. The anaesthesia has mounted full one inch higher in the trunk since the last examination, but it has not yet extended to either of the upper extremities. The engorged condition of the lungs is gaining headway, the harassing suffocative cough has returned, and the whole chest is drenched in perspiration. The nurse says that the patient has only just recovered from a distressing attack of hiccough. Temperature of right arm, beneath the shirt and flannel, 90°, of inner aspect of right thigh, 90°, of right leg, 90°, of the trunk in several places, 88°. The

lower extremities slightly moist. No evident disposition to bed-sores. On passing the catheter the patient intimated that he felt some uneasiness in the urethra. He also said that this was the first time since the commencement of the illness in which this operation had given rise to any sensation. Urine still quite acid.

Jan. 11th.—No material change. Certainly no improvement, and if anything, the engorgement of the lungs somewhat increased. Urine acid.

12th.—More exhausted. No sleep in the night, the increased difficulty of breathing attending sleep causing him to awake immediately if he for a moment forgot himself. No marked change in other respects.

Half-past four P.M.—He felt the catheter pass into his bladder, and at the same time an obscure feeling informed him of the passage of flatus from the rectum.

Half-past seven P.M.—The bowels have just acted voluntarily, and he was distinctly conscious of a stool having passed by means of the ordinary sensations in the anus. Urine acid; distinctly so.

Jan. 13th.—Death happened at seven A.M.

Post-mortem examination, (Jan. 14th, half-past four P.M.)—Rigor mortis fully established everywhere; the skin of the back, upon which the corpse had been lying, presenting considerable signs of suffigation, especially along the course of the spine. The arachnoid covering of the cord everywhere is clear, smooth, and without any traces of inflammation. The outside of the lumbar enlargement is curiously nodulated. On making a longitudinal section of the cord, the whole substance, from the brachial enlargement to its inferior termination, is found to be of a yellowish-red colour, softened in a remarkable manner, and in the lumbar region almost like cream in consistence. Several small patches of extravasated blood are scattered in the softened structure; these patches are undefined in outline, more numerous in the lumbar than in the dorsal portion of the cord, and situated chiefly in the posterior columns. The red discoloration of the cord which has been mentioned is most marked in the neighbourhood of these patches. The examination did not extend further, the friends of the patient only consenting to it on condition that it should be thus partial.

Jan. 15th.—On examining some portions of the diseased cord under the microscope, the natural structure is found to be altogether broken down, and mixed with blood corpuscles, exudation granules, and, in fewer number, pus-corpuscles.

Dr. Radcliffe says:—"In this case there are no insuperable difficulties in diagnosis to clear up. There are no "head-symptoms," properly so called, to complicate matters; and all the symptoms are such as must be referred to loss of function in the spinal cord from the attack of some acute disease. The paraplegic character of the paralysis in the first instance—the way in which the paraplegia crept upwards, attacking both sides at the same time and to the same level—the going of the anæsthesia hand in hand with the paralysis over the same regions—point plainly to the spinal cord. The rapid course of the disease, the absence of convulsive or quasi-convulsive symptoms at the onset, the presence of paralysis and anæsthesia in company, and especially the implication of the bladder and rectum in this paralytic anæsthetic condition, seem to point to acute disease of the substance of the cord, rather than to acute disease of the spinal meninges. The history of the case evidently agrees in the main with what is

known of acute general myelitis, and this was the disease diagnosed at the time.

"But, it may be asked, ought not the urine to be alkaline in myelitis? That it is said to be so, is plain; that it was not so in this case, is not less plain. This is the only answer which can be given to this question. And yet the post-mortem examination shows that the case in question is as pure a case of acute myelitis as one is likely to meet with.

"Nor can it be supposed that another symptom in the case under consideration—erection of the penis—is a certain proof that the membranes of the cord are implicated in the disease; for in this case the most careful examination failed to find any ground for supposing that the membranes of the cord were otherwise than perfectly healthy. And this is a point of some importance, inasmuch as there is a commonly received impression that erection of the penis is not present in uncomplicated cases of acute myelitis.

"It would not be difficult to find many points upon which to comment in the case under consideration; but I limit myself to three—the absence of pain or tenderness in the back; the absence of any change in the iris and in the bloodvessels of the eye; and the return of some degree of sensation in the penis, bladder, and rectum, when the cord was evidently disorganized in a very high degree, if not utterly.

"It appears to be strange that there should have been no pain or tenderness in the course of the spine; at the same time it is certain that the same absence has been noticed in other cases of the kind, and that these cases cannot be regarded as exceptional. So that the present case is only an additional piece of evidence to the same effect.

"The absence of any change in the iris and in the bloodvessels of the eye is an interesting fact. It may be looked upon as a reason for believing that the disease in the cord at the time of the ophthalmoscopic examination did not extend high enough to implicate the cilio-spinal region; for it may be presumed that if this region had been implicated, the iris would have been more opened or closed, and the vessels more dilated or contracted than usual. This may be presumed; for recent physiological experiments have shown that the iris and the bloodvessels of the eye are affected in the same way by acting upon the cilio-spinal region of the cord as that in which they are affected by acting upon the great sympathetic nerve-cords of the neck.

"And, lastly, what of the return of some degree of sensation in the penis, bladder, and rectum, in the two days preceding death, after it had been altogether absent for upwards of a week, and when it is certain, from the condition of things found after death, that the spinal cord must have been in a state of total disorganization? I could find a good deal to say on this subject if I had time. As it is, I will only say that there are not a few cases on record in which a considerable portion of disorganized spinal cord has not barred the transmission of nervous impressions. The pathological facts are not

to be disputed; nor is their interpretation altogether out of the question. There are several physiological experiments by the great Humboldt, to which I have referred elsewhere,* which show that "nervous influence," be that what it may, can be transmitted across a considerable gap in a nerve. And there is a curious case which may be mentioned here as bearing upon the same point—a case which seems to show that in order to the re-transmission of nervous impressions along a divided nerve, all that is necessary is to bring the two ends together again. This case is that of a man who was admitted on the 13th of June last into the Hôtel Dieu at Paris, under the care of M. Laugier, for a serious wound of the forearm. This wound had divided, amongst other things, the median nerve. The next morning the two ends of the divided nerve were brought together, and kept in that position by passing a needle carrying a silk thread through each end, and then tying the thread in a knot. The operation was performed under the influence of chloroform, and after the anæsthesia passed off, the patient seemed to be quite at ease. On the evening of the same day the patient was able to feel—indistinctly, it is true—the contact of objects applied to the parts which before the operation were altogether devoid of sensibility. The next day the return of feeling was more evident, and the thumb could readily be brought into apposition with the other fingers. A day or two later neuritis was set up in the injured nerve, and for a while the progress of the case was not altogether satisfactory; but all went right in the end, and at no distant period the once divided nerve had recovered altogether its natural functions. This case is full of interest in many respects; to us now it is of interest as showing how a divided nerve may transmit impressions in both directions soon after the divided ends are brought into contact, and before there could be any proper organic connexion between these ends. This is a point of interest to us now; for if this be so, and if the experiments of Humboldt to which I have referred be true, then it ceases to be so very exceptional a matter that nervous impressions may still continue to be transmitted in cases where the continuity of the tubules of a nerve-centre is destroyed by disease. How this transmission is effected I do not attempt to explain here: I have made the attempt elsewhere. Here I only insist upon the fact with a view to suggest the possibility of very considerable restoration of function in cases even in which it is certain that considerable portions of the spinal cord or brain have lost, it may be permanently, their healthy structure. I wish, indeed, to enter a protest against the commonly received notion that organic damage in a nerve-centre is irretrievable; and I think I am justified in doing so. At any rate, I have certainly seen some remarkable recoveries of function in cerebral and spinal disease which would be difficult to account for if there could be no such recovery without complete organic reparation of the damaged parts."

* Lectures on Epilepsy, Pain, Paralysis, and certain other Disorders of the Nervous System. Delivered at the Royal College of Physicians of London. Churchill, 1864.

ART. 42.—*On the Nature and Treatment of Uræmic Convulsions.*

By Dr. D. RUTHERFORD HALDANE, Physician to the Royal Infirmary, Edinburgh.

(*Edinburgh Medical Journal*, April, 1865.)

The principal conclusions arising out of Dr. Haldane's investigations are these :—

"1st. In the present state of our knowledge we seem justified in believing that the retention of urea is the chief cause of uræmic convulsions; though the effect of this agent is probably aided by individual peculiarities, and by deficient nutrition and consequent irritability of the brain, the result of the hydræmic condition of the blood.

"2nd. It seems probable that the mass of the blood may be increased when deficiency of the urine is not compensated for by augmentation of other secretions, or by the occurrence of dropsy, and that this condition predisposes to apoplectic symptoms.

"3rd. In cases where symptoms of cerebral congestion are present, especially where well-marked contraction of the pupil exists, blood-letting is likely to do good.

"4th. In cases where symptoms of cerebral congestion are absent or ill-marked, and where dilatation of the pupil exists, bloodletting is likely to be injurious.

"5th. Chloroform is valuable as a palliative, especially in cases of puerperal convulsions; but when symptoms of cerebral congestion are present, it should not alone be depended on."

ART. 43.—*On a New Lesion of the Brain in General Paralysis.*

By M. REGNARD.

(*Annales Médico-Psychologiques*, Janvier, 1865.)

Dr. Baillarger, on examining the brain of insane individuals who had suffered from paralysis, noticed long ago that, in some cases, when the grey substance of the anterior lobes after the removal of their membranes, was scraped with the back of a scalpel, prolongations or ridges of the white matter stopped the knife, and were cut away with some difficulty. The white matter could be thus exposed, and looked of a firm consistency, and somewhat yellowish. Some of its prolongations, elastic and tough, resembled the epiglottis in colour and appearance. In other words, a process of induration seemed to have gone on in the most superficial layers of the medullary substance, whilst the deeper ones retained their normal consistency.

M. Regnard endeavoured to ascertain whether this lesion was

constant, or at least frequent, and whether, in all cases, it was possible to make out its presence or absence. He made, for this purpose, 12 post-mortem examinations: in 8, he found the lesion well-marked; in 3, it was indistinct and incomplete; in 1, it was totally absent. In 5 of the 8 cases in which the lesion was well-marked, the disease had not lasted more than nine months—a fact which shows that this lesion exists in the first stage of general paralysis. In 20 of the same class of cases, the white matter was softened beneath the superficial layers that were indurated. The obvious conclusion is, that the lesion is entirely restricted to the superficial layer of the white matter. In the cases comprised in the second category, there was very evident softening of the white matter; whilst in the single case of the third series, there was not softening only, but actual diffuence of the white matter. It would appear, therefore, that the lesion is all the more marked, in proportion as the brain substance is of diminished consistency. All the patients whose brains were examined had, during life, suffered from embarrassment of speech, and M. Regnard draws attention to the fact, long ago pointed out by Bouillaud, that the anterior lobes were diseased; and also, that the third left frontal convolution was, in all the cases, as much disorganized as the rest.

ART. 44.—*A Case of Tracheotomy in Hydrophobia.*

By Dr. NORMAN CHEEVERS.

(*Indian Med. Annals*, January, 1865.)

Dr. Cheevers gives many quotations from various authorities as to the expediency of tracheotomy in hydrophobia, and relates at length a case of this kind (Abstract xxx. p. 50) in which Mr. Scriven carried out the same treatment. There is, however, reason to believe that Mr. Scriven's case was not one of true hydrophobia, and that, on this account, it is wanting in a great part of the interest which belongs to the present case:—

CASE 1.—Redya Chand Dutt, aged twenty, Hindoo, a household servant, a native of Burdwan, a slight but tolerably muscular and healthy looking young man, was admitted to the Calcutta Medical College Hospital at half-past one o'clock in the afternoon on the 29th of April, 1863. The patient's uncle stated that, about two months previously, he was attacked in the road by a strange dog and severely bitten on the outer part of the left ankle, under the malleolus. The wound was very slow in healing, not being completely cicatrized until about a fortnight before his admission. He was not certain whether the dog was mad or not. He took many native remedies of reputed value as prophylactics of hydrophobia. Four days before admission, he suffered from a sense of general uneasiness, with some obscure pain in the back and limbs, and with slight heaviness of the head and distaste for food.

On the day previous to his admission, at about ten o'clock A.M., he attempted to take his morning meal, but was prevented from swallowing by a kind of choking sensation in the throat. With the occurrence of this symptom, the pain in the back began to increase and that in the limbs changed

from a dull to a gnawing character. During the night, the pain in the back gradually extended upwards to the neck, which became and continued somewhat stiff. At about seven o'clock on the evening previous to his admission, he succeeded in swallowing a few drops of water, but with great suffering. During that night, he slept very badly, suffered a good deal from pain in the back and limbs, and was attacked five or six times with spasms. At about six o'clock on the morning of his admission, a sensation of cramp about the pharynx became troublesome and continued. The principal symptoms on admission were, gnawing pains in the limbs and a dull pain in the back. Stiffness about the neck and suffocative paroxysmal spasms in the muscles of the larynx and pharynx, pain in the thorax, and a sense of tightness below the ribs. When spasm commenced about the pharynx, a peculiar pain was experienced about the epigastrium. It was then evident that the whole of the muscles of respiration and deglutition were seriously involved. The spasms recurred every ten or twelve minutes; and, during their continuance, he suffered from much difficulty in breathing. The breathing was much easier during the intervals. There was a peculiar wild and anxious expression of countenance. Forehead beaded with perspiration. Pulse small and quick, 90 in the minute. No urine or stool to-day. Is hungry, and has great thirst, but cannot swallow anything, each attempt to swallow exciting suffocative spasms. Is not affected by the sight of liquids. Spasms are now and then excited by touching the body. Surface of natural temperature. There is no hawking or increased secretion of saliva, but he, from time to time, licks his lips as if they were sticky. There is a raised callous cicatrix, two inches long, about an inch below the outer malleolus of the left ankle. It is neither tender nor sensitive under pressure, nor is it the centre of any radiating pain.—Ordered half a grain of morphia in powder immediately, to be repeated in half an hour and then to be continued every hour.

Six P.M.—The suffocative spasms have much increased in severity, respiration much more embarrassed. The surface of the body and the limbs is bedewed with a cold sweat. Is becoming very irritable. Pulse small, quick, and occasionally irregular.—Continue the morphia.

As the evening advanced, all the worst symptoms increased in severity, and the patient's strength gradually failed under the progressively augmenting severity of the spasmodic paroxysms. The pulse became smaller, slower, and more irregular; great pain in the neck was complained of. I now began to entertain the conviction that the operation of tracheotomy afforded the patient the only chance of valid relief. While watching the advancing severity of the spasms I made the following note:—

"I am by no means certain that this patient is threatened with death by spasm of the glottis, considering that he has said repeatedly that he feels no uneasiness in that situation during the spasms, and that there is nothing very distinctively suffocative in the character of the spasms. His chief danger appears to be from asthenia. He has a quick, small, intermitting and irregular pulse, and he has not taken food either yesterday or to-day. Still, must there not always be something suffocative in the character of such spasms as these? May not opening the trachea enable him to swallow? Would it not be a great advantage, at least, to ensure him against all possible risk of death from sudden asphyxia caused by closure of the glottis, or from gradual exhaustion aggravated and hastened by attacks impeding respiration, and, thereby, still further disturbing the brain and heart."

At a quarter past ten o'clock at night I performed the operation of tracheotomy. The patient was persuaded to undergo it without much difficulty, and bore it exceedingly well. It was rapidly and easily done,

and was not interrupted by any considerable spasm ; the fullest sized curved trachea tube available was introduced without bleeding, although a small integumentary artery threw out a florid jet. The freest respiration through the opening was immediately established. As had been anticipated, the relief produced by the operation became immediately evident. Although a spasm came on whenever ice was given him, he took small lumps into his mouth with great avidity. *He also took several table-spoonsful of cold water.* The spasms, however, increased in violence with every spoonful, his eyes protruding, his hands quivering, all the muscles of his neck working like cords, the air rushing through the canula in sibilant blasts, and almost forcing the pipe out.

It was manifest that, previous to the operation, every attempt to drink created violent spasmodic action in three sets of muscles, (1) the pharyngeal, (2) the laryngeal, (3) the diaphragm and thoracic muscles of respiration. The operation immediately allayed and put a final stop to the injurious effects of contractile spasm in the glottideal muscles. Consequently the patient was at first enabled to swallow liquids, subduing, with difficulty, the resistance of the pharyngeal and respiratory muscles. The spasmodic action in these two sets of muscles, however, gradually obtained the ascendancy, and deglutition became more and more difficult at every draught. My observation of this patient's condition convinced me that, while tracheotomy in hydrophobia effectually prevents suffocation from spasm of the glottis, it is still possible that suffocation may occur from spasm of the thoracic muscles of respiration and diaphragm. This, however, was not the issue of my patient's case.

The effect of the operation was to afford him very considerable temporary relief, to enable him to quench his thirst, and to secure him an interval of relief from attacks of spasm. Half a grain of morphia was given after the operation (making four and a quarter grains taken within about nine hours). This medicine was ordered to be continued in quarter grain doses every hour. At a quarter to twelve o'clock, an hour after the operation, he was very quiet, and had been comparatively free from spontaneous spasms, having only been moderately attacked twice. He took a dose of morphia without embarrassment, *and swallowed six table-spoonsful of strong soup in succession.* The first spoonful was gulped down at once with great avidity, overcoming strong spasm. In fact now, as before the operation, spasm attacked him at every attempt to swallow liquids, but he could now contend with it, and swallow in despite of it, as it could not now completely strangle him and stop respiration. With each spoonful, however, the violence of the spasms increased : the last of the six produced extreme distress.

He took ice pretty freely, although he was attacked with spasms whenever it was brought near him. The pulse had become slower, 84 in the minute, but much fuller than before the operation ; rather bounding and distinctly unequal, but without intermission.

12:40 o'clock.—Has been lying nearly free from spasms, and very tranquil, but has not slept. When attempts are made to cover his face with a sheet, slight spasmodic action is produced. I now sent for some wine, but the sound of moving a spoon in the wine, which had been poured into a tin mug, produced most violent spasms, one of the worst attacks that he had yet experienced. He could scarcely be held on the bed, uttered a distressing short moaning cry, the air passing with great force and loud sibilation through the canula. The attempt to give him wine was, therefore, immediately suspended. In the absence of spasm his pulse was regular, but some of the beats were perceptibly above and others below the average strength.

1 A.M.—*Has taken five or six spoonsful of water with much difficulty. Has*

had a few spasms, during which respiration was much embarrassed, although air passed freely through the artificial opening. The mind is quite clear.

2½ A.M.—Started in a spasm when I came near his bed. *Has taken a table-spoonful of sago.* Gets spasms every few minutes. In the severest of these, excited by taking his dose of morphia (bringing the quantity taken up to five grains), he started back and clung to his uncle's arm, with an expression of the most intense and supernatural terror, giving a vivid idea of one spectre-stricken. Pulse constantly varying, at one moment rapid and almost regular, at another slower and very irregular; no longer complains of thirst; cold perspiration on forehead and extremities.—To continue the morphia every hour, providing the intellect remains clear. To have chloroform freely poured on his pillow.

3¼ A.M.—Paroxysms, not of very extreme severity, occur about every three minutes. During a moderate paroxysm, the pulse becomes more irregular than when he is at rest. Sometimes faster, at others slower.—To have fluid food if it can be swallowed without extreme distress. Chloroform to be freely used, so that the patient's head may be constantly surrounded with its vapour. The tube continues perfectly free.

5 A.M.—*Has taken a spoonful of milk* since last report; the frequency of the spasms is increased. Has had no sleep throughout the night. Pulse small, very quick, and occasionally intermittent. Had a severe spasm just now, becoming suddenly convulsed throughout the whole body. During this paroxysm he tried to get up from his bed. His mind is not so clear as before.—Continue the morphia.

8 A.M.—Had several severe spasms since the last report, four or five during the last half-hour. Took some wine and water without much difficulty. Respiration much quicker than before. Chloroform inhalation was employed. The chloroform appeared to give great relief. It rendered him unconscious of pain, and, when he recovered from the effects, he said he felt better. The first effect of the chloroform was to increase spasm, but, when continued, it certainly gave relief; he became unconscious, and no doubt free from all suffering, and thus obtained temporary rest from the great exhaustion caused by the constantly recurrent spasm, and consequent pain and nervous exhaustion. Cold sweats on the forehead and the limbs. Extremities cold. Pulse small and frequent.

9½ A.M.—Is getting spasms every five minutes. Has not taken any wine or water, but took his medicine with some difficulty. Is getting delirious. Pulse small and very frequent. Makes a moaning noise, and grinds his teeth; now and then he tries to rise up from the bed. Is very restless.

10¼ A.M.—Spasms more frequent than before. Is delirious, grinding his teeth and attempting to rise from the bed. Extremities cold and covered with sweat, foams at the mouth, and makes a terrible noise; pulse barely perceptible at the wrist. The spasms extend to the muscles of the extremities. Is in the utmost suffering.

11 A.M.—Strong soup was injected into the upper part of the rectum by the long tube, but it was expelled by spasmodic action. The eyes have a very furious expression, spasms more frequent than before, sweats all over the body. Is making a terrible noise. Is very restless, pulse small and barely perceptible at the wrist; some foam at the mouth.

12 NOON.—Extremities cold and clammy. Pulse nearly imperceptible, spasms not so afflicting and violent as before.

12½.—No pulse, spasms weaker than before, extremities cold; cold sweat all over the body.

1 P.M.—Died by asthenia.

No post-mortem examination could be made, as the friends insisted upon removing the body immediately after death.

"This case," says Dr. Cheevers, "must be regarded as one of considerable interest, it being, as I believe, the first in which the operation of tracheotomy, proposed by Dalziel, Herbert Mayo, and Marshall Hall, has been tried in an undoubted case of hydrophobia in the human subject.

"It is valuable, as proving that, in a case where the horror of liquids was most intense, *opening the trachea enabled the patient to drink.*

"It proves that, where death by spasm of the glottis is rendered impossible, the patient may still sink by asthenia from the violence of the spasms, especially of those affecting the thoracic muscles of respiration and the diaphragm.

"I regard Mr. Scriven's case as one of, so-called, idiopathic hydrophobia, in which life was saved by the early performance of tracheotomy. I view my own as one in which tracheotomy was had recourse to early enough to produce decidedly beneficial effects; but (as spasm of the heart had previously set in and asthenia was advancing) too late to preserve life.

"I feel convinced that the pain of the operation added scarcely anything to the sum of the patient's agony, and was infinitely more than compensated by the power of quenching thirst, and by the very decided, though only temporary, relief of spasm and partial restoration of cardiac power which followed.

"I should never hesitate to perform tracheotomy in any case of hydrophobia in which death by spasm of the glottis appeared to be threatened.

"As preventing frequently-recurring attacks of exhausting trachealismus, this operation must, when performed early, be regarded as tending to avert death by asthenia.

"I am firmly of opinion that, whenever a case of hydrophobia is seen early, the surgeon is justified opening the trachea immediately.

"This operation demands care on the surgeon's own account. Considering how often slight abrasions on the hands and face pass unnoticed, the risk of being freely sprinkled with the blood and sputa of a hydrophobic patient is by no means trivial—M. Decroix, a French veterinary surgeon, having proved, by experiment, that the saliva of a man labouring under hydrophobia is capable of exciting rabies in the dog."

ART. 45.—*On Sclerosis of the Lateral Columns of the Spinal Cord, in an Hysterical Woman whose four Limbs had, during Life, been permanently contracted.*

By DR. CHARCOT.

(*Gazette Hebdomadaire de Médecine et de Chirurgie*, Février 17, 1865.)

This case formed the subject of a paper read before the Société Médicale des Hôpitaux, of Paris, 25th January, 1865. The author

began by stating that, up to the present time, the alteration known by the name of sclerosis, or grey degeneration of the spinal cord, had been only studied in cases of progressive locomotor ataxy, and in that complaint the lesion is almost exclusively limited to the posterior columns of the cord, leaving the anterior and lateral columns in a nearly normal state. In the present case, on the contrary, the posterior and anterior columns were healthy, whilst the lateral columns, on both sides, for a great portion of their thickness and throughout their length, from the medulla oblongata as far as the lumbar swelling, were the seat of the grey degeneration. Several of the anterior roots were also atrophied, but the posterior roots were all normal. There was no trace of spinal meningitis. The characters of the sclerosis were very distinct, the columns had a greyish, semi-transparent, gelatiniform look; their consistency was greater than usual; their substance was infiltrated with a transparent amorphous or fibrillary substance, through which were scattered nuclei of connective tissue and corpora amylacea. Lastly, there was atrophy of the nerve tubes which presented a series of dilatations and constrictions. The grey matter was healthy. The nerve cells were normal, as in the cases of locomotor ataxy, previously examined by Drs. Vulpian and Charcot.

Sclerosis of the lateral columns has been recorded several times, among others, by Dr. Türk (Academy of Sciences of Vienna, 1856). Dr. Charcot once met with it in a case, the history of which he could never learn. This lesion, therefore, is not altogether rare, and more carefully made post-mortem examinations will, doubtless, multiply instances of this new pathological species. Up to this time, the cases on record have been chiefly deficient in accurate clinical observation. The present case of Dr. Charcot's, however, is less deficient than the rest in this respect.

The patient had, from the age of 14, presented the most characteristic symptoms of convulsive hysteria. Later, the convulsive fits became less frequent, and were replaced by permanent motor disorders. Thus at the age of 34, after an hysterical fit, she suffered from contraction of the left arm and leg, which lasted a fortnight, and then disappeared all of a sudden. A year afterwards the same limbs became contracted, and after a time, the right limbs were similarly affected. For two years, she was obliged to remain almost completely motionless, with scarcely a few short intervals of intermission. At the end of that time, a marked improvement set in spontaneously, and the patient was able to walk about and attend to her house. In 1855, however, another violent fit brought on contraction again of all the limbs and the muscles of the trunk. From that time the condition remained persistently until 1864, when an inter-current affection carried her off. Her intellect was never impaired, up to the very last.

ART. 46.—*On Acute Creeping Paralysis.*

By Dr. PELLEGRINO LEVI.

(Arch. Gén. de Méd., Février, 1865.)

Dr. Levi has brought together, in this Memoir, fourteen cases of acute creeping paralysis, one of which he had occasion to study personally, at the Lariboisière Hospital, in Dr. Pidoux's wards, and the rest are borrowed from the works of Ollivier (D'Angers), Cruveilhier, Landry, Kussmaul, Lizard, and Duchenne.

An analysis of these cases disclosed the following facts:—1. Prodromata are frequently present, which last from a few weeks to a few days, and even a few hours only. These consist in tingling, chiefly felt in the toes and fingers, and more or less marked weakness of the extremities, the lower ones principally. This sensation of fatigue sometimes increases by degrees; but in some cases—in fact, in most cases—it suddenly becomes notably intense, without any appreciable cause, and after a very short interval is transformed into real paralysis. In the vast majority of instances the paralysis assumes, at first, the form of paraplegia. It is not common to find one limb much more affected than the other, and it is only in very exceptional instances that the upper limbs, or even the pharynx and œsophagus, are first affected.

The upper limbs, if not paralyzed simultaneously with the lower ones, next become implicated; but Dr. Levi noted that, even in the very last hours of life, the forearms seem to preserve very feeble movements. The patient constantly lies on his back, with his legs extended; the muscles are thoroughly relaxed.

The next symptoms are those of paralysis of the muscles lodged in the vertebral grooves of the diaphragm, pharynx, and œsophagus, &c. The patient is no longer able to move in his bed; he complains of difficulty of breathing, of pain in the epigastrium; and it is found, on inspection, that the epigastrium sinks, instead of rising, during inspiration.

The dyspnoea generally varies in intensity; and the deglutition of solids, and even of liquids, sometimes produces in the patient the sensation of being out of breath. Dysphagia sets in two or three days or more after the invasion of the paralysis: in two instances, it was one of the first symptoms noted. It varies also, in degree, on successive days.

The muscles seem to retain their sensibility, and this has even been found augmented in some cases. At no period of the disease are contractions met with, spasmodic jerks, fibrillary vibrations, or tremor. In the few cases in which they were tested, reflex movements were found either very diminished or completely abolished. In two cases, electro-muscular contractility was unimpaired; in one it was considerably diminished. General sensibility is far from being as seriously impaired as motility. The various kinds of sensibility are retained, and there scarcely exists numbness of the sole of the foot. The tingling, noticed at the outset, persists and sometimes

extends upwards. The absence of spontaneous or elicited pain along the spin has been always noted. The senses are not affected, as a rule, or very slightly so. Speech, without being actually embarrassed, is sometimes thick; the movements of the tongue or lips are in such cases less free than in health, but without accompanying tremor.

Intelligence and memory are always perfect. The patient sometimes complains of general uneasiness and of insomnia, but never of headache.

The only symptom noted, as regards the digestive organs, is a more or less troublesome costiveness. Micturition, even in those cases, has been easily and normally performed. The pulse never rises above 100, and is often less; there is moderate warmth of the body, but profuse and general perspiration is a very frequent symptom. Lastly, death supervenes, and sometimes very rapidly, through immobility of the diaphragm and ribs.

The *course* of the disease is generally continuous, rapidly extending from below upwards; there may be true intermissions, however, and the disease may thus last several months, although such cases are exceptional. On an average, death takes place about the eighth or tenth day. In nine out of fourteen cases, death occurred within a few days.

When recovery takes place, it is generally very slow, and begins in the muscles which have been last affected. The *causes* of the disease are almost unknown. In many cases, exposure to cold, when the body is warm and perspiring, has been noted, the disease making its appearance a few hours after. Dysmenorrhœa, or sudden cessation of the menses, have in two or three instances been met with. As to the *pathological anatomy* of the affection, nothing is yet known. The brain and spinal cord have always been found normal, even on microscopical examination.

ART. 47.—*Differential Diagnosis between Disease of the Cerebellum and Tabes Dorsalis.*

By M. DUCHENNE (de Boulogne).

(*Gazette Hebdomadaire de Médecine et de Chirurgie*, No. 46, 1864.)

M. Duchenne (de Boulogne) formerly believed that these conditions could not be distinguished by the gait of the patient; but more exact observation has convinced him of the contrary. The movements in cerebellar disease precisely resemble those of intoxication. A drunken man, when standing, wavers towards all sides, in a degree corresponding to that of his drunkenness. The sufferer from tabes makes quick short movements, like those of a rope-dancer or balancer, to preserve his equilibrium; and these movements, at first instinctive, in time become voluntary. In advanced stages the patient with tabes cannot support himself when erect, but can move freely when recumbent, which the drunken man cannot do.

The drunkard progresses in curved lines, but each step is natural; while the tabetic patient advances in a straight line, but with spasmodic steps, in proportion to the degree of incoordination. He is fearful of falling, and does not venture to look away from the ground. The drunkard feels dizziness, and his difficulties are in his head; but the tabetic feels no dizziness, and refers his uncertainty to his feet.

ART. 48.—*Case of Cruveilhier's Atrophy ending in Cure.*

By Dr. RADCLIFFE, Physician to the Westminster Hospital, and to the National Hospital for Paralysis and Epilepsy.

(*Lancet*, January 14, 1865.)

The common impression is, that the disease of which this case is an example is one which sooner or later ends fatally. This is the impression, for example, which is gathered from the perusal of M. Trousseau's excellent clinical lectures on the subject, seeing that not one single word is said in this lecture on remedies or cure. Moreover, the very epithet *progressive*—in the name, progressive muscular atrophy—implies, more or less distinctly, a fatal tendency. In fact, however, there are other cases on record which show that a less gloomy impression is justifiable; and that this is so, abundant proof may be found in the excellent work of M. Duchenne (de Boulogne) on "*Electrisation Localisée*," and in the model essay of Dr. Roberts on "*Wasting Palsy*."

CASE.—Caroline B—, aged nineteen, of Ticehurst, Sussex, a domestic servant, admitted into the National Hospital for Paralysis and Epilepsy on the 24th of March, 1864.

Present State.—The patient complains of weakness and wasting of the left hand, and says there is nothing else the matter with her. She also says that this wasting and weakness began five months ago only.

In the left hand, the muscles forming the ball of the thumb (the thenar group) are not more than a third of the size of the corresponding muscles in the other hand; and the hollowness of the metacarpal interspaces is such as to show that the interossei and the lumbricales muscles have disappeared in great measure. The special muscles of the little finger (the hypothenar group) are also considerably wasted. The fingers are curved in the position which belongs to incipient "bird's-claw deformity," or "*main en griffe*"—the deformity which M. Duchenne (de Boulogne) has shown to be the direct result of atrophy of the interossei and lumbricales muscles. The wasted muscles present no signs of fibrillary or other involuntary movement. Those belonging to the ball of the thumb contract freely under the action of the magneto-electric current; but no sign of contraction can be produced in this way in the spaces where the interossei and lumbricales muscles ought to be. No numbness is complained of at the present time, and none can be detected by the compasses. Comparing the left hand with the right, the surface is found to be cooler, bluer, and moister in the former than in the latter. The pulse at the wrist is also more feeble on the left side than on the right. The muscles of the left arm and of the rest of the body present nothing wrong in form or function; indeed, with the exception of the condition of the left

hand which has been described, the patient (who is a short, healthy-looking, well-developed florid girl, with dark hair, light-grey irides, and large but regular pupils) would seem to be in perfect health, both bodily and mental.

Previous History.—Nearly five years ago the patient noticed that her left hand was weak, wasted, and occasionally numb. About the same time the little finger became bent, and got “into the way;” and from that time to this the condition of the hand has gone on gradually altering into that which exists at present. Latterly, she says, the hand may have “gone wrong a little more quickly.” She also says that from the first the fingers would frequently “die,” particularly when cold, and recover with “pins-and-needles.” Of late, also, there appear to have been occasional attacks of palpitation.

Previous to the commencement of the wasting of the left hand, the patient appears to have enjoyed uniformly good health; and she distinctly says that she never sprained or hurt this hand in any way. Her family history is also good. She is one of ten brothers and sisters, all living and well, as she believes, except three, who died of fever; and both her parents are living.

Treatment.—Faradization, kneading, and suitable localized movements to be used every day. Half a fluid drachm of syrup of iodide of iron to be taken thrice a day, with full diet and porter.

Progress of the Case.—April 9th.—All the treatment carried out. Faradization has been used eleven times. Slight action in the lumbricales and interossei muscles.

20th.—Is now able to straighten the first and second fingers, and to do something towards straightening the third and fourth. The hand is warmer to the touch. The Faradization has been repeated eighteen times.

May 13th.—The hand has almost entirely recovered its full form and functions. The Faradization has been repeated twenty-six times.

June 23rd.—The wasted muscles have now fully recovered their bulk and uses. The form of the left hand is now perfectly natural, and its bulk quite equal to that of its fellow. The patient, in fact, has perfectly recovered, and is to-day discharged *cured*.

ART. 49.—*On Scrivener's Palsy.*

By Mr. SOLLY, Senior Surgeon to St. Thomas's Hospital.

(*Lancet*, Dec. 24, 1864, Jan. 28, and Feb. 4, 1865.)

Mr. Solly makes a case of this disorder a text for a clinical lecture. He is of opinion that the disease depends upon granular degeneration of some of the ganglionic cells of the spinal cord, in the region of the brachial enlargement. But he cannot yet support this opinion by appealing to the disclosures of post-mortem examination. Speaking as to treatment, he says:—

“My own experience in the treatment of these cases is very decided: rest—entire rest from the occupation that has produced the disease. If the disease is recognised early, then two months' entire rest will arrest its progress; at the end of that time strychnine is of service, but it is injurious when given in the early stage of the disease, and before rest. During the time of rest the health should be sustained and improved in every possible way. Fresh bracing air—mountain air if attainable—is of more service than all the tonics

in the world. As regards tonics, they must be varied to suit the peculiarities of each constitution. Zinc, iron, and quinine are the tonics on which I rely most."

ART. 50.—*Remarks on Defects of Expression (by words, writing, signs, &c.) in Diseases of the Nervous System.*

By Dr. HUGHLINGS JACKSON, Assistant-Physician to the National Hospital for Epilepsy and Paralysis, and to the London Hospital.

(*Lancet*, November 26, 1864.)

In a former paper, of which we gave an abstract in another volume (xxxix. p. 108), Dr. Jackson tried to show the association of hemiplegia (nearly always in the right side) with loss of speech and valvular disease of the heart. In the present communication he points out, that when with hemiplegia on the right side a patient *talks* with what appears to be but a slight defect in articulation, there is frequently considerable defect in the power of expressing ideas in other ways, especially in writing.

Many words have recently been coined to express the defects of speech met with in practice. As, however, they are more often defects of expression generally than of speech alone, and again, as these defects vary most widely in degree, there is a risk that attempts at precision in names might confuse rather than help inquiry. No single word can define defects which differ so widely from one another. The important point in practice, for purposes of diagnosis, is to determine their clinical association. Now defects of language, whether general or particular—in the widest or in the narrowest sense of the word, (excluding, of course, inability to talk from loss of power in the machinery of articulation—i.e., lips, tongue, &c., and incoherence)—agree in this, that they are found with hemiplegia on the right side, and not with hemiplegia on the left. To this rule, however, Dr. Jackson has recorded three exceptions, one of which occurred in his own practice. This mutual agreement of the symptoms must be kept as carefully in view as their individual differences. Thus if, as at one extreme, a patient cannot utter or write a word, and if, besides, he cannot make a sign by way of reply (loss of the general faculty of language); or if, as at the other extreme, he has simply a difficulty in uttering words (defect of articulate language)—the clinical associations are just the same. But there are good grounds for believing that the differences in the symptoms, great as they are, are but divergences in different directions, and to varying extents, from one common point. For although merely defective utterance may, in a given case, be the sole marked feature of loss of language, and though the patient may be able to write and make signs, yet it will be found that he does so badly and with very great difficulty. In nearly all cases, then, the *general* faculty of language is somewhat impaired, although in some it is shown nearly altogether by defect of articulate language, and but slightly

by defect of other modes of language, as by signs, writing, &c. Recently Dr. Jackson has adopted the simpler word "expression," in preference to "speech" or "language." It defines less, and yet covers more. And if it is not, from its looseness, of any value as a scientific definition in psychology (although the real central thing in all kinds of language is to *express* intellectual propositions), yet it may be safely used as a term in clinical investigation. In each case, however, the defect ought to be described as it affects language by words, by writing, and by signs.

ART. 51.—*Paralysis of the Lips, Tongue, and Palate.*

By Professor GERHARDT.

(*Jenaische Med. Zeitschr.*, p. 197, 1864.)

In this case, during the life of the patient, Gerhardt had diagnosed the existence of a circumscribed lesion of the pons. The symptoms were complete speechlessness, with impeded movement of the tongue. The lips and teeth could be closed, but whistling and blowing were impossible, and there was paralysis of the vocal cords and epiglottis. Sight, hearing, and mind were unaffected. Occipital pains and hemiplegia on the right side were gradually developed, and after frequent attacks of threatened suffocation, the patient died comatose two years after the commencement of his illness. The autopsy showed recent softening in various spots of the cerebrum and cerebellum, the sixth, facial, auditory, vagus, and hypoglossal nerves somewhat smaller on the right side than on the left. A section through the highest portion of the cord showed the grey substance disorganized, in the right anterior column a patch of softening, the lateral and posterior columns of a general reddish grey colour. In the pons was a brownish violet coloured spot, the size of a pea, gradually fading into the surrounding tissue, and with vascular dilatation visible to the naked eye. The microscope shewed that the vessels were dilated in a spindle-shaped outline. The paralysed muscles were well nourished and dark coloured; the left side of the tongue only being somewhat paler than the right. It is probable that the paralysis of the extremities was due to the softening of the cord, and the paralysis of the lips, palate, and tongue, to the disease in the pons, where the part affected corresponded to the crossing point of the facial nerves. Gerhardt has also seen a second case of buccal paralysis, in which the speech can be rendered intelligible for a time by a continuous galvanic current, passed from the angle of the lower jaw to the soft palate.

ART. 52.—*Paralysis of the Tongue, Palate, and Vocal Cord.*

By DR. HUGHLINGS JACKSON, Assistant-Physician to the London Hospital, and to the National Hospital for the Epileptic and Paralysed.

(*London Hospital Clinical Lectures and Reports*, vol. i. 1864.)

CASE.—Paralysis of the right side of the tongue, with wasting. Paralysis of the right side of the palate, and of the right vocal cord. Slight weakness of the limbs, especially on the right side. Laryngoscopic examination by Dr. Morell Mackenzie.

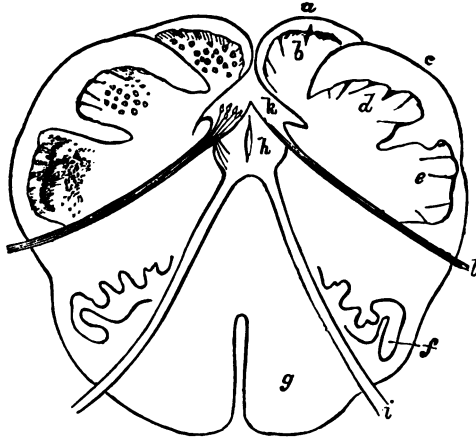
Thomas C—, aged fifty, was admitted, under my care, at the Hospital for Epilepsy and Paralysis, March, 1864. One morning, six months ago, he found that he had lost his voice, or, to give his own words, that he “was hoarse,” and that “his tongue was turned to the right side like a hook.” He could whisper anything he had to say quite well, “but his voice was lost;” there was no shrillness about it. He had had difficulty in swallowing, and “soreness” of the throat for six weeks before these decided symptoms, but no hoarseness. The difficulty in swallowing became gradually greater. He did not give up work for six weeks. In March, when I first saw him—i. e., six months from the onset of the paralysis—he had the following symptoms:—Complete paralysis of the right side of the tongue, with much wasting, and it deviated to the paralysed side. The fold of the palate did not rise so well on the same side, but was dragged up to the left. He had lost his voice, and could not cough, or rather, he could not shut the larynx in coughing, but coughed like a patient does whose trachea had been opened. The right shoulder was much lower than the other, and he could not shrug up this shoulder nearly so well as the other. There was no obvious paralysis in any part of the face, but the patient said he could not whistle so well, and this, perhaps, indicated loss of power in the orbicularis oris. He could articulate in a whisper very well.

Commenting upon the cause which produces at one and the same time paralysis of the tongue, palate, and vocal cord, all on one side (and doubtfully of the orbicularis oris), Dr. Jackson says:—

“Now the recent researches of Lockhart Clarke supply us with an exceedingly simple and satisfactory explanation of the connexion of these three symptoms.

“Lockhart Clarke has remarked, that, according to Bendz and Claude Bernard, the *lower* rootlets of the spinal accessory nerve are collected into the *external* branch which supplies the trapezius and sterno-mastoid muscles; while the *upper* rootlets go to form the *internal* branch which joins the pneumogastric, and is through it distributed to the larynx, pharynx, and palate. Now, the *lower* roots, forming the external branch, have been shown by Lockhart Clarke to arise, in common with the anterior roots of the spinal nerves in the cervical and brachial region, from the anterior grey substance of the spinal cord; while the *upper* roots, forming the *internal* branch of the spinal accessory, have an entirely *different* and a *double* origin—one from its own special nucleus, continuous behind the central canal with that of the pneumogastric; the other from the proper nucleus of the lingual or hypoglossal nerve, in

front of the canal. On the other hand, some of the fibres of the lingual nerve *appear* to take their origin from the spinal accessory nucleus."



Transverse section of the human medulla oblongata, opposite the lower part of the olivary bodies. *a*, Posterior pyramid; *b*, Grey substance within it. *c*, Restiform body; *d*, Grey substance within it. *e*, Expanded extremity of the posterior horn of the spinal cord—the caput cornu, or grey tubercle of Rolando. *f*, Olivary body. *g*, Anterior pyramid. *h*, Grey nucleus of hypoglossal nerve; between and behind this and its fellow of the opposite side is the small fusiform canal. *i*, Hypoglossal nerve, passing out between the olivary body and anterior pyramid. *k*, Grey nucleus of the spinal accessory nerve. *l*, Spinal accessory nerve passing out from its nucleus, immediately in front of the caput cornu or grey tubercle, *e*. On left side of Fig., some of the roots of this nerve are seen entering its own nucleus, *k*, and others turning forward into the hypoglossal nucleus at the part corresponding to letter *h*.

The author then goes on to say :—

"Thomas C—, though he has lost his voice, can breathe, which, of course, he could not do without great difficulty if *all* the motor nerves of the larynx were paralysed. 'At every inspiration the glottis opens and allows the air to pass freely into the trachea; at every expiration it collapses, and the air is driven out from below.' These movements are called 'the respiratory movements of the glottis' (Dalton). And as the larynx has two functions, vocalization and respiration, so there are, according to many physiologists, two sources from which the muscles receive their motor power. The pneumogastric by the superior laryngeal supplies the crico-thyroid, and by the inferior the other muscles of the larynx. But as the pneumogastric is a sensory nerve at its origin, it must derive its motor power from other nerves. The spinal accessory is said to be

the one which joins the pneumogastric, for the movements in the larynx concerned in *vocalization*, whilst the pneumogastric obtains the motor fibre which it gives to the larynx for the movements of *respiration* from other sources."

In order to obtain more precise information as to the laryngeal symptoms, Dr. Morell Mackenzie was requested to examine the larynx; and the following is his report:—

"It was observed that on attempted phonation, the right vocal cord scarcely moved at all, whilst its fellow advanced well to the median line. A space was therefore left between the cords, and the aphonia was due to this cause. Neither forced expiration nor coughing appeared to affect the immobility of the right cord. The inward movement of the right arytenoid cartilage was likewise in abeyance, but on attempted phonation this cartilage, together with the tubercle of Wrisburg, and the capitulum of Santorini, moved to and fro from before backwards. There was no atrophy of either of the cords."

Dr. Jackson continues:—

"The above case shows the association of paralysis of three parts, which, although distinct in position, are usually combined in the same actions. No one would—*à priori*—suppose, then, they are (from Clarke's researches) explainable on the supposition of centric disease. Cases of paralysis of the tongue, palate, and vocal cords on both sides, are not very uncommon; but cases in which these parts are paralysed on one side only, are rarely met with. The suddenness of the attack, the patient's age, and the presence of albumen in the urine, render it probable that the lesion was owing to an effusion of blood; and the absence of any great paralysis of the limbs shows that that disease must have been limited, and—as seems most probable from the researches of Clarke—on one side of the medulla, near the nuclei of the spinal accessory and ninth nerves. Post-mortem examination, in cases so definite as this, would be of great value, not only in pathological but in physiological researches.

ART. 53.—*Neuralgia of the Lingual Nerve cured by Electricity.*

By DR. NAFFE.

(*Allg. Med. Zeitung*, No. 24, 1864; and *Gaz. Hebdomadaire de Médecine et de Chirurgie*, Janvier 13, 1865.)

CASE.—A man, aged thirty, after exposure to a draught, felt very acute pain in the posterior and left lateral region of the mouth. The pain, although continuous, was more intense at times, especially at night. It always began in the same spot, near the last molar, and extended to the tip of the tongue. Mastication was painful, and the patient was unable to take any solid food. The pain next spread to the greater portion of the mouth. After having tried various modes of treatment, Dr. Naffe had recourse to electricity. The anastomosis of the lingual nerve with the chorda tympani led him to act on this latter branch. For that purpose, he filled the external auditory meatus with water, and placed in it one of the conductors of the pile, resting the

other against the mastoid process. The continuous electric current had scarcely passed through the ear than all trace of pain disappeared. A few more sittings, on the following days, brought on a complete cure within a short time.

ART. 54.—*Symmetrical Atrophy of both Posterior Lobes of the Cerebrum.*

By Dr. WAGNER.

(*Arch. d. Heilk.*, v. p. 371, 1864.)

CASE.—An idiot from birth, of cheerful disposition, unable to read or write, died in his twenty-ninth year from inflammation of the lungs. Wagner found the posterior lobes of the cerebrum quite rudimentary, so that the cerebellum projected about two inches beyond them. The rudimentary lobes, in section, were greyish white, firm, smooth, and homogeneous, almost without distinction of cortical or medullary substance. The other parts of the cerebrum and the lateral ventricles were imperfectly developed—the corpora striata, thalami, pons, and medulla oblongata were normal. The microscopic appearances of the atrophied parts resembled those of inflammatory sclerosis.

ART. 55.—*On Spontaneous and Reflex Muscular Tension and Rigidity.*

By Dr. BENEDIKT.

(*Deutsche Klinik*, No. 46, 1864.)

Dr. Benedikt has described two conditions which he finds not to be unfrequent in cases of nervous disease. They are:—1. Spontaneous muscular tension usually occurring where the motor nervous system has been greatly exhausted, and producing functional disturbance liable to be mistaken for paralysis; and, 2. Muscular rigidity excited by passive movements. He describes and illustrates them at considerable length, and shows that they may be either centric or excentric in their origin. Their treatment resolves itself into that of the various diseases in which they occur; and this, in the hands of the author, is usually by some form of electric current.

(B) CONCERNING THE RESPIRATORY SYSTEM.

ART. 56.—*On the Characters of the Expectoration in Cases of Fetid Bronchitis and Gangrene of the Lungs.*

By Dr. ARTHUR GAMGEE.

(*Edinburgh Medical Journal*, March, 1865.)

In this paper Dr. Gamgee relates the particulars of a case of gangrene of the lungs, under the care of Dr. Haldane, in the Royal

Infirmary, Edinburgh, in which butyric acid was found in the sputum, and he concludes that this fact is fatal to Professor Laycock's opinion that the presence of butyric acid in the sputum is characteristic of fatal bronchitis, as distinguished from gangrene of the lungs. He also asserts, as a fact which he has recently made out, "that butyric acid, or one of the homologous volatile fatty acids, is present in the sputum of almost all, if not all, cases of chest disease; in other words, that the sputum always, or nearly always, contains a volatile acid. He says:—

"On taking the most odourless expectoration of a case of simple acute, or chronic bronchitis, or the mucous expectoration of early phthisis, or the muco-purulent expectoration of the advanced disease, acidifying with sulphuric acid and boiling the fluid, vapours are evolved which have an acid reaction to test paper, and possess in a marked manner the odour of one of the volatile fatty acids—generally of butyric acid. If the process be carefully carried on in a retort, and the distillate be neutralized with an alkali, evaporated to dryness and then acidulated, a fluid is obtained possessing in a marked manner the odour of the fatty acid occurring in the sputum—the odour being the best available test which we can employ in the detection of small quantities of these acids; being as characteristic as it is delicate.

"On examining the sputum of a patient recovering from acute bronchitis, I discovered that the acid which was evolved, after acidifying and boiling, had very much the odour of caprylic acid. The muco-purulent sputum of a case of phthisis yielded an acid vapour which had at first the odour of formic acid. On boiling the fluid farther, a butyric-acid odour was evolved. The acids which I think occur in the sputum are butyric acid, propionic acid, formic acid, acetic acid, and possibly caprylic acid. The occurrence of these acids in the sputum is not a matter for surprise. The researches of Scherer, Gorup-Besanez, and Schottin, have proved the existence of most of these in the muscular juice, milk, &c., which most probably derive them preformed from the blood. Formed by the oxidation of various fatty matters, they are probably constantly present in small quantities in the blood, where they are partly consumed, and partly separated by the various excretory organs, and, like other volatile substances, especially by the lungs. When the air passages contain fluid it is only natural that they should be dissolved by the fluid, and thus find their way into the sputa. It will be a subject for future inquiry whether the quantity of volatile acids in the sputum is not greatest in those cases where the function of the lungs is so much impaired as to interfere materially with the oxidation of the blood.

"The above observations answer, it appears to me, in a satisfactory manner, the questions which I purposed solving in this communication;—they show that the occurrence of butyric acid in the sputa cannot at present be proved to have any semeiological value; and that its presence is in no way characteristic of fetid bronchitis.

"In reference to fetid bronchitis, it appears to me that the facts which have been collected with regard to it are scarcely of such a

nature as to warrant our admitting a disease bearing this name into our nosologies. Like all other excretions, that of the air passages is occasionally liable to exhale a disagreeable odour. This sometimes occurs in acute and chronic bronchitis. No distinct and uniform series of phenomena have, however, been shown to characterize those cases of bronchitis in which the expectoration is fetid. We should, I think, avoid a needless and unphilosophical complication of our nosologies were we to speak of a bronchitis accompanied by fetid expectoration, instead of a disease *sui generis* "fetid bronchitis."

ART. 57.—*On Dilatation of the Bronchi.*

By Professor SKODA.

(*Wien. Allg. Med. Zeitung*, No. 35, 1864.)

In some clinical lectures on this subject, Professor Skoda dwells upon the differences that such dilatations present, in respect of character, course, and result—differences, the most important of which depend upon the size of the dilatation, the extent of destruction of the lung tissue around, and the condition of the mucous membrane. Bronchial dilatation differs essentially from pulmonary phthisis, in that the former is not a disease, but only a result of former morbid processes. There are cases, however, in which it closely simulates phthisis, and especially the form called *phthisis pituitosa* by old writers.

Bronchial dilatation may exist for a long time without very seriously affecting the health, although it always entails liability to attacks of acute bronchial catarrh, and increases the discomfort which such attacks occasion. In cases where the mucous membrane of the dilated tubes is the seat of extensive ulceration, with profuse purulent discharge, emaciation and hectic, or even pyæmia, may be produced; and, in the latter case, the metastatic abscesses often contain pus having the aspect and peculiar odour of the bronchial secretion. Bronchial dilatation and tubercular phthisis are not incompatible, as was supposed by Laennec, but may co-exist.

Dilatation of the bronchi depends upon three chief causes, namely: loss of natural resistance, inspiration, and accumulation of secretion. The disease which chiefly produces it (interstitial pneumonia) is fortunately less common in mankind than in cattle.

The diagnosis rests chiefly upon the character of the sputa. Percussion is only of value when the parenchyma between the dilated tubes is condensed and without air, and when the tubes so surrounded are large and numerous. The cracked-pot sound will then be heard as over a cavity, with the addition of a kind of râle, produced by the movement of air and fluid in the tubes. Auscultation is of no value unless frequently repeated, since it is only the *continuance* of consonant râles and bronchial breathing that will afford evidence in the case. The sputa, when the disease is at all extensive, are always characteristic, in consequence of the dilution of the purulent portion

by a watery fluid, giving to the whole a curdled appearance. More characteristic still is the odour of sebaceous acid. When auscultation and percussion show that such a secretion as this is furnished by the lower portions of the lungs, the presence of dilatation is highly probable; and may be considered certain, if these lower portions are resonant. When the secretion is furnished by the upper lobes, the diagnosis from phthisis is difficult or impossible.

The best treatment is the inhalation of vapour impregnated with turpentine or with tar. Salines and balsamic remedies may also be useful, and soluble antimonials may be given occasionally to promote expectoration. From astringents Skoda has never seen any direct action.

ART. 58.—*On the Production of Vibrios and Bacterias in Inflammation of the Bronchi, &c.*

By M. POUCHET.

(*Journ. de Méd. et Chir. Prat.*, Dec. 1864.)

Having frequently remarked that in inflammatory affections of the trachea and bronchial tubes, and also in those of the nasal mucous membrane and of the auditory duct, the patients after a good night's rest often complain of intense itching in the diseased parts, M. Pouchet suspected that the cause of the irritation might be analogous to that observed at the margin of the anus when oxyuri exist in the intestines, and might possibly arise from the presence of microscopic animalculæ; and further inquiry has confirmed the accuracy of this suspicion.

On two separate occasions, a man suffering from slight pulmonary catarrh awoke after a perfectly quiet night with a sensation of intense titillation in the trachea; M. Pouchet examined the first sputa thrown up, and found a large amount of extremely lively bacteria, mixed with monads. Copious expectoration followed, and in the course of half an hour the irritation ceased, and the animalculæ disappeared from the secretion.

M. Pouchet made the same observation in a case of coryza.

In another subject affected with otorrhœa, this gentleman, on eight or ten separate occasions, remarked that whenever the discharge was scanty and formication distressing, the secretion invariably contained a large number of animalcules, chiefly bacteriæ, mixed with monads and vibrios. When the suppuration was abundant and unattended with itching, no living infusoria were discoverable.

Hence, the author concludes that when, under peculiar circumstances, the secretions of the mucous membranes or of some parts of the skin are altered in their nature by inflammatory action, bacteriæ, vibrios, and monads are generated, and that the unbearable pruritus complained of chiefly arises from the movements of the former, which are always present in large numbers. The phenomenon ceases when the animalcules disappear or perish. No infusoria

exist in the healthy secretions of the bronchi, ear, or nasal fossæ. Their development coincides with a morbid change in these liquids, induced by heat and moisture, which promote putrefaction of the fluids thrown out by mucous surfaces.

ART. 59.—*The Therapeutics of the Chronic Catarrhal State.*

By Dr. J. G. GLOVER, Surgeon to the North Holloway Dispensary.

(*Lancet*, March 4, 1865.)

"The state under consideration," says Dr. Glover, "is one in which the more stimulating expectorants have been chiefly used, and undoubtedly in a considerable number of cases they do good service. Finding, however, many cases in which these medicines did not afford much relief, I cast about for another remedy. Observing the efficacy of dilute nitric acid in a case of whooping-cough, and being struck with the fact of a nervous element in the cases under consideration, evidenced by the paroxysmal character of the cough, its being troublesome mostly in the night, and the well-known efficacy of different agents whose influence is chiefly through the nervous system, I resolved to treat a few cases with the nitric acid, and without the expectorants. My general prescription has been: Dilute nitric acid, ten minims; spirit of nitrous ether, twenty minims; syrup, one drachm; peppermint water, an ounce and a half. Mix. To be taken every six hours. The result has been a valuable amount of relief—a less frequent cough, often a return of the ability to lie in bed, a more prolonged and continuous sleep, and general improvement of the patient. I refrain from all dogmatism. A more extensive experiment may qualify my own opinion, and still more that of others, of the value of the suggestion I make. I have, of course, discovered that it is not universally applicable. The state of chronic bronchitis is eminently one for eclectic treatment, according to its complications, the age and constitution of the patient, &c. I only know that in a certain number of cases the above prescription has afforded important relief after the failure of more common medicines, and that the season is opportune for any remarks which tend to increase our resources, in the management of so common a disease as chronic bronchitis."

ART. 60.—*On a Case of Pneumothorax without Liquid Effusion.*

By Dr. SCHROTTER.

(*Gaz. Hebdomadaire de Méd. et de Chir.*, Mars 17, 1865.)

CASE.—This case, which was brought before the Medical Society of Vienna on the 23rd of last December, is that of a woman, aged thirty-five, who

had suffered from pneumothorax, which had been afterwards cured, without any signs of liquid effusion having been ever detected. There had been antecedent hæmoptysis; and the patient was carrying a trunk up a staircase, when she felt a sudden and violent pain in the left side of the thorax, and a sensation as if a warm liquid was flowing back from the stomach to the chest. She was admitted into the hospital a fortnight after this. The left side of the thorax was bulged considerably, the diaphragm was pushed down, and the heart's apex displaced inwards. Pneumothorax was, in fact, easily diagnosed; and Dr. Schrotter thought that it had resulted from a tubercular cavity opening into the pleural sac. By degrees the signs of pneumothorax disappeared, and the respiratory murmur became audible again, although it was at first very weak and indistinct. At the end of eleven weeks the air was completely absorbed, and auscultation detected pleuritic friction sound.

ART. 61.—*On the Temperature of the Body as a Means of Diagnosis in Phthisis and Tuberculosis.*

By DR. SIDNEY RINGER, Professor of Materia Medica
in University College, London.

(Pamphlet, London, p. 90, 1865.)

Dr. Ringer is of opinion that the temperature of the body will be found to be an important aid in the diagnosis of phthisis and tuberculosis, and he relates 25 cases as examples of the facts upon which he founds this opinion. The subject is certainly one which demands further and fuller investigation; for, to say the least, Dr. Ringer affords good reasons to believe that the thermometer may do good in clearing up some obscurities of diagnosis in the cases in question.

The following propositions are considered in turn, and they will serve to show the precise drift of the pamphlet:—

"1. There is probably a continued elevation of the body in all cases in which a deposition of tubercle is taking place in any of its organs.

"2. This elevation of the temperature is probably due either to the general condition of the body (tuberculosis), or to the deposition of tubercle in its various organs (tubercularization).

"3. This elevation is probably due to the general condition (tuberculosis), rather than to the deposition of the tubercle (tubercularization).

"4. The temperature may be taken as a measure of the amount of the tuberculosis and tubercularization, and any fluctuations in the temperature indicate corresponding fluctuations in the severity of the disease.

"5. The temperature is a more accurate indication of the amount of tuberculosis and tubercularization, than either the physical signs or the symptoms.

"6. By means of the temperature we can diagnose tuberculosis and tubercularization long before the physical signs and symptoms are sufficient to justify such a diagnosis.

"7. By means of the temperature we can diagnose tuberculosis even when during the whole course of the disease there are no physical signs indicative of tubercular deposit in any of the organs of the body, and in which cases the symptoms (apart from the temperature) are inadequate to enable us to arrive at such a diagnosis.

"8. It is probable that by means of the temperature we can conclude that the deposition of the tubercle has ceased, and that any physical signs that are present are due to obsolescent tubercle and the chronic thickening of the lung-tissue between the tubercular deposit.

"9. It is probable, though further observations on this point are necessary, that the temperature of the body affords a means by which we can diagnose between diseases in which the symptoms and physical signs are either too scanty or too much alike to enable us to decide between them."

ART. 62.—*Dermoid Cyst of the Lung.*

By Dr. S. W. SALOMONSEA.

(*Bibliothek für Læger*, Jul. 1863.)

CASE.—The case is that of a servant girl, twenty-four years old, who died after long illness, with symptoms resembling those of pulmonary phthisis. The right lung was closely adherent to the thorax, contained abscesses, and was greatly modified in structure by chronic disease. Near its root was found a dermoid cyst, as large as a pigeon's egg, lined with distinctly-organized skin, containing sebaceous matter and small brown hairs, and communicating with a bronchial tube. The cyst was precisely like those found in the ovaries, and which contain teeth, hairs, or epithelium. The author states that there are only two other cases on record in which such cysts have been found in the lungs.

ART. 63.—*On the Influence of the Climate of the Pyrenees on Pulmonary Phthisis.*

By Dr. PIETRA-SANTA.

(*Gaz. Hebd. de Méd. et de Chir.*, Février 17, 1865.)

The author adopts the following conclusions:—

1. The air breathed in the Pyrenees, at a height varying from 700 to 800 metres, at a barometric pressure of 700 millimetres, possesses special properties: (a.) It is naturally lighter. (At an altitude of 1000 metres, the lungs of a man of medium height take in, under the same volume and for the same amount of thoracic expansion, air which has lost one eighth of its normal density and weight. At Eaux-Bonnes, the loss is equivalent to 38 litres of air per hour, or 912 litres per day). (b.) The air contains less oxygen for an equal volume. (This diminution of oxygen amounts to 23 milligrammes per litre, or 11 grammes per hour, and 264 grammes per day). (c.) It

contains a larger proportion of aqueous vapour. (Observations made by the author himself, with Saussure's hygrometer as well as August's psychrometer, prove that the hygrometric curve is constantly found in the highest degrees of the scales.) (d.) It contains a good deal of ozone, that is to say, of oxygen in a particular electric condition. (At all portions of the day and night, the violet or blueish tints of the prepared papers of Tame, of Sedau, and of Houzeau, of Rouen, are most strikingly developed.)

2. An atmosphere thus constituted exerts a beneficial influence on chronic affections of the respiratory organs. (The proofs of this assertion are based on analogy, direct experiments, and clinical observation.)

3. This atmosphere, therefore, becomes a very powerful auxiliary, in the way of treatment, to the sulphuretted mineral waters scattered throughout the country.

ART. 64.—*The Indications for Paracentesis Thoracis.*

By M. MARROTTE.

(*British Med. Journal*, Nov. 19, 1864.)

After considering (1) the accidents attending paracentesis thoracis as an operation; and (2) the information we possess regarding sudden death during an attack of pleurisy, the frequency of its occurrence, its causes, and the means of preventing it; M. Marrotte investigates (3) the indications for performing paracentesis in—*a.* Acute and chronic serous effusion; *b.* Sero-sanguinolent effusion; *c.* Purulent effusion.

Acute and Chronic Serous Effusion.—In cases of pleurisy, attended with excessive effusion, paracentesis is absolutely necessary when asphyxia is imminent, whatever may be the concomitant symptoms. But when the respiration and circulation are not markedly impeded, the immediate performance of the operation is not necessary, even though the viscera be notably displaced, and the heart even pushed beyond the middle line; by delaying it too long, however, there is danger lest mental emotion or physical effort may produce rapidly fatal syncope or pulmonary congestion.

If there be slow asphyxia, or acute asphyxia, as in cases where the ascent of the effused fluid is rapid; if dyspnoea be evident to the patient, or to the physician alone; if the circulation be impeded; if the pulse be unequal, irregular, intermittent; if syncope be present or be threatened—the operation should be performed, even when there is no displacement of the heart. This precept is strongly insisted on by M. Marrotte, with one limitation. Effusion may take place rapidly, and produce dyspnoea, without compromising life; the lung is surprised for the moment, but soon becomes accustomed to the new condition in which it is placed.

The presence of effusion is almost unanimously considered to be a condition necessary for operation; but some practitioners regard

the mere presence of effusion itself as a sufficient indication for paracentesis; while, with others, it must have reached a certain degree and produce certain symptoms.

Acute Serous Effusion.—It has been recommended by some practitioners to puncture the pleura at a period varying from the seventh to the eleventh day; while others advise that the operation should be delayed until the fifteenth or even the twentieth day. But, says M. Marrotte, those who thus lay down absolute rules as to time, forget that all cases of pleurisy do not run through their stages in the same period; that their rise and progress are subject to conditions which vary in each case; that the cause of the effusion is not removed by the operation; and that of itself it cannot, beyond its physical effect, fulfil the necessary indications of treatment. This is so true, that M. Béhier, one of the advocates of early operation, after recommending the ninth or eleventh day for its performance, recognises the impossibility of precisely fixing the proper moment. M. Marrotte seeks indications in the state of the disease itself rather than in general arbitrary rules. Except in cases where death is imminent, he holds that it is impossible, at a period varying from the seventh to the eleventh day, to affirm that a serous effusion, the result of latent pleurisy, will not yield to medicinal treatment.

The presence of active inflammation is a contra-indication to the operation; if, however, the symptoms be very urgent, puncture of the chest may be resorted to in order to procure temporary relief, but it will not prevent the reproduction of the fluid.

It is generally admitted that paracentesis is neither necessary nor useful in recent cases, where the effusion is moderate in quantity; but some maintain that, where the effusion, although not so great as to produce danger, is yet considerable, the operation may shorten the duration of the medicinal treatment, and prevent the formation of solid adhesions. M. Marrotte, however, cannot regard paracentesis as presenting great advantages over ordinary treatment in these cases. It may be that dyspnoea and febrile symptoms have ceased, as if by enchantment, after paracentesis; and evacuation of effused serum has been proposed as an antiphlogistic remedy in inflammation of the tunica vaginalis and of the eye; but this practice must be justified by more numerous and better-studied facts before it can be generally adopted. M. Marrotte acknowledges, however, that he has met with cases where early paracentesis has appeared to shorten the duration of medicinal treatment, and to prevent contraction of the chest.

In the same category with very abundant effusion, as regards the effects produced on the respiration and circulation, may be classed those cases where, in addition to effusion, some impediment to respiration, such as bronchitis or oedema, exists in the opposite lung; and cases of double pleurisy, especially when complicated with pericarditis. In such cases M. Marrotte agrees with M. Béhier in admitting the utility, and even the necessity, of paracentesis.

M. Béhier recommends the fluid to be evacuated when the patient seems too feeble to be able to bear the long process of absorption

of an effusion occupying the whole, or nearly the whole, side of the chest. But, in cases of this kind, M. Marrotte says, the probabilities of non-reproduction of the fluid must be considered: for evacuation of serous cavities tends indirectly to induce exhaustion, and to produce the very result which is sought to be avoided.

M. Marrotte confirms a statement made by M. Béhier, that the presence of pulmonary tubercle is not an absolute contra-indication to paracentesis. He has performed the operation in a patient with pulmonary tubercle and ascites, who was in danger of suffocation. The fluid was partly reproduced; but, under the influence of tonics, cod-liver oil, nutritious diet, and the external use of tincture of iodine, the effusion, both in the pleura and the peritoneum, disappeared. The patient, however, died a year afterwards of tubercular disease of the meninges. In two or three other cases, the pleurisy and the pulmonary tubercle went on to a fatal result. In one other case only, where pulmonary tubercle co-existed with considerable effusion, M. Marrotte obtained a successful result by paracentesis. As far as can be judged, the cases of this description where paracentesis is likely to succeed, are those in which the tubercles are stationary, or undergoing very slow development; and where the pleurisy, whether symptomatic or accidental, is of the latent form. On the other hand, where the phthisis and the pleurisy are acute, puncture is not only useless, but hastens the transformation of the effused fluid into pus.

Sero-sanguinolent Effusion.—The presence of blood in pleural effusion is generally connected with cancer or tubercle of the pleura; hence an unfavourable prognosis must generally be pronounced, not only as to the issue of the malady, but as to the immediate result of the operation. There are, however, cases on record in which, even though the fluid has had a reddish colour, recovery, even rapid, has followed paracentesis; but in these the idea of pleural cancer, or tubercle, cannot of course be entertained. Although these favourable cases are rare, it is important to be able to recognise them. Independently of other circumstances indicating the absence of constitutional disease, they are distinguished, M. Marrotte thinks, from sanguinolent effusions symptomatic of organic lesion, by the small proportion of blood in the fluid. Sanguineous effusions connected with cancer or tubercle are generally more coloured, the presence of blood is more distinctly marked, and the fluid which escapes towards the end of the operation more or less resembles pure blood.

Chronic Serous Effusion.—The results of paracentesis appear to have been generally unfavourable in cases of chronic effusion; and hence some reject the operation, without denying that cures are possible. Cases in which recovery has followed have been cited by M. Woillez; but our judgment is at fault, when we inquire when chronicity commences, what cases of chronic effusion are likely to receive benefit from puncture, and how they may be recognised. Chronicity has generally been defined according to the duration of the disease rather than according to its progress and symptoms. But, M. Marrotte observes, a distinction must be drawn between

those cases where the disease is still active, and often ends in the development of tubercle or pus, and those where there is serous effusion, properly so called, the simple remains of the disease. Chronic pleurisy—that is, where the pleuritic process is still active—comes much more under the domain of medicinal treatment than of paracentesis.

Of effusions which are met with as the remains of pleurisy, some have followed more or less active inflammation, accompanied by the formation of plastic products which have become organized and have produced impediment to the expansion of the lung. In these cases, puncture will probably fail if delayed for two, three, or six months; but if the history of the case lead to the supposition that there has been acute dropsy of the pleura, or simple latent pleurisy—in both which the plastic products are small in quantity—there is a chance that the lung will expand on the evacuation of the fluid, although the effusion is of long duration.

Purulent Effusion.—In regard to cases of this kind, M. Woillez has noticed two circumstances which have also occurred to M. Marrotte: viz., the readiness with which pleurisy passes on to suppuration in children where it becomes chronic; and the frequency with which puncture with the trocar is followed in them by pleuro-cutaneous fistula. When empyema is present, the formation of a subcutaneous fistula, so as to allow the exit of pus, and the ultimate evacuation and contraction of the cavity without allowing the entrance of air, is much preferable to repeated punctures; although these have been followed by successful results in the hands of Legroux and Roger. M. Marrotte's recommendation has reference to children; but whether it will succeed equally well in the adult must, he observes, be determined by future experience.

(C) CONCERNING THE CIRCULATING SYSTEM.

ART. 65.—*On some Functional Disorders of the Heart.*

By Professor LÖSCHNER.

(*Prager Vierteljahrschrift*, t. 84, 1863; and *Gaz. Hebd. de Méd. et de Chir.*, Janvier 27, 1865.)

The author calls attention to certain cardiac symptoms which he has, in some cases, observed both in children and in adults, as the result of exposure to cold immediately after a bath. The symptoms, he says, are those of myocarditis; namely, violent, unceasing, and occasionally irregular palpitations, fluttering, prolongation of the heart-sounds, violent pulsations of the carotids; often, oppression, anxiety, want of air. General nutrition seems to be deeply affected, and considerable wasting sets in rapidly. Whatever be the nature of these phenomena, they are extremely serious, and are often quickly succeeded by paralysis of the heart and lungs, and even when the patients do not die, they undergo the most agonizing pain.

Dissection, in fatal cases, showed spots of circumscribed myocarditis and lobular pneumonia, pericarditis or endo-pericarditis. The author adds further, that anasarca is of frequent occurrence in the last days of life. The best method of treatment consists in the use of cooling medicines, digitalis, and perfect rest, cold lotions over the præcordial region, warm bland drinks taken in large quantities, deep inspirations frequently repeated, and low diet. These symptoms are much more dangerous in children than in adults, and in the latter, they become exceptionally serious when they show themselves in individuals already affected with a grave dyscrasia, such as syphilis, for example. Dr. Löschner lays particular stress on this last point, and draws to it the attention of physicians attached to thermal establishments.

ART. 66.—*On Auricular-Systolic Murmur in Cases of Contracted Mitral Orifice.*

By Dr. GAIRDNER, Physician to the Glasgow Royal Infirmary, &c.

(*Medical Times and Gazette*, March 18, 1865.)

In a clinical lecture, delivered at the Glasgow Royal Infirmary, in February last, Dr. Gairdner says:—"Many of you have heard me explain the importance of the pre-systolic, or, as I call it, the *auricular systolic* murmur, as indicating a contracted mitral orifice, of which lesion, indeed, it may be said to be pathognomic, if we exclude a few cases of tricuspid obstruction, and some of disease of the pericardium, in which a more or less similar murmur may be present.

"I show you here the heart of Jas. M'L., who died a few days ago in Ward 7, and whose body was examined, in my absence, by the Resident Physician, Dr. Wyber. As the mechanism of the heart was found to be considerably deranged, it was preserved for your inspection. You observe that while the heart, as a whole, is very considerably enlarged, weighing, on being removed from the body and emptied of blood, twenty-eight ounces, or more than double the proper weight for the heart in a man of his size, the left auricle is dilated much out of proportion to the other cavities; so much so, that, with a little stretching, it would probably hold a very large orange. You have, on the other hand, the cavity of the left ventricle of a moderate size. With regard to the auriculo-ventricular valves, you observe that while the tricuspid orifice, without any other marked deformity, is somewhat widened, admitting nearly four fingers, the mitral is very much contracted, and does not fully admit the point of even one finger. The pulmonary and aortic semilunar valves are normal, though the aorta itself is rather small, owing probably to the relatively small amount of blood passing through the left auricle. The only valve that is the subject of distinct disease, therefore, is the mitral, and that disease is obstruction. I do not say that there was no regurgitation in this case. I

believe there was. But as the two lips of the valve come pretty accurately together, and as the small size of the mitral orifice is due not to vegetations, but to a contracted state of the orifice, regurgitation, if present, must have been slight in amount, and entirely subordinate to the obstruction. One other fact that illustrates the condition of the circulation in this heart is the thickening and loss of translucency of the left auricle as compared with the rest of the endocardium, showing a stasis of the blood in this auricle; and in further proof of this stasis of the blood, you observe that the auricular appendix is blocked up by a clot which, from its consistency, density, adhesive and granular character, has evidently been formed during life, and probably long before death. You have, therefore, multiplied proofs in this case that there was a very decided obstruction in that mitral orifice, the only question being whether there was not also a slight amount of regurgitation through the same orifice; and, in connexion with this, I wish to read you the facts of the case relating to the murmur, as reported in the journal at the time.

"This man was twice under observation. He came into hospital for the first time on September 13th of last year, and was dismissed improved on November 18th. During this time he was subjected to very minute, careful, and repeated examination. He was readmitted on February 9th, 1865, and he died on the 25th of the same month; but this time he was so ill that he was only examined sufficiently to satisfy my own mind that the physical signs were the same as on his first admission. In the journal of like date, the murmur is described as a 'distinct and protracted auricular-systolic murmur, followed by brief ventricular-systolic, at left apex.' The murmur, as you observe here, beginning at a brief interval after the second sound, extends close up to the first sound, and there is a very short, almost doubtful murmur after the first sound, but no murmur directly in contact with the second sound. Next day we noticed that 'the ventricular-systolic element in the murmur is to-day indistinct and doubtful; otherwise the facts are the same as yesterday. The cardiac dulness is somewhat extended both to left and right; greatest transverse diameter is probably nearly six inches, but not quite distinct at right border.' There was great dilatation of the heart, therefore, as ascertained by percussion, quite in consistency with what you observe here. We find also the following note:—'Tested at the apex, the first sound is decidedly the sharper of the two, inasmuch that it might easily be mistaken for the second. Tested towards the base, the second sound regains its natural relation to the first, but develops a distinctly reduplicated character, which even at the apex serves to identify the sounds.' Now, I wish to tell you that it sometimes happens in these cases that the first and second sounds are rather difficult to distinguish from each other on account of this peculiarity, frequent in cases of mitral disease, that the second sound is very much increased in intensity at the base over the pulmonary artery, while the first sound is somewhat similarly increased in intensity and sharpness at the apex, where, indeed, it often assumes very much the

character of the second sound. But when the first and second sounds are in this manner difficult of distinction, though you can generally get out of the difficulty by employing a little extra care, the best way, beyond all question, is to employ this instrument, the differential stethoscope of Dr. Scott Alison. Putting one bell of the stethoscope over the apex and the other over the base of the heart, the erroneous impressions as to the sounds of the heart formed by the one ear are corrected by the other. In this case, however, the reduplication of the second sound was of great assistance in enabling us to identify the sounds both at the base and apex, and thus to come to a proper understanding of the rhythm of the heart.

"You see, then, that we made a very careful examination of this case, and what made me particularly anxious that you should observe the facts of the murmur for yourselves was the circumstance that this was one of three cases which I published some time ago in the *Medical Times and Gazette*, and in regard to which I said that 'if any one of these three cases should turn out to be other than a case of mitral (or just possibly tricuspid) obstruction, I shall be the first to confess that the whole pathology and diagnosis of the subject, as I have hitherto viewed it, requires reconstruction.' Now, one of these three cases died while my communication was in the hands of the printer, and the state of the heart is described in a foot-note.* The case of J. M'L. is therefore a second illustration of the significance of the auricular-systolic murmur in cases of mitral contraction; and if any of you wish to examine the matter still more closely, by referring to the journal of the ward you will find an exact description of the murmur as heard during life.

"The third case which was noticed in the paper in the *Medical Times and Gazette*, viz., that of Mary O'M., has been dismissed from the hospital improved; but we shall very likely see more of her. If she comes back, you must not allow your scientific curiosity to make this girl the subject of uneasy inquiry; but I shall endeavour, if she is in a fit state for examination, to give you further opportunity of becoming acquainted with the auricular-systolic murmur. There is, however, another case of this murmur in the ward at present, and yet another case was only recently dismissed. These cases, indeed, are not at all rare, as you might suppose from

* This case was an exceedingly complicated one, and undoubtedly a difficult diagnosis. The murmurs were auricular-systolic and ventricular-systolic, with an occasional ventricular-diastolic murmur heard at the base and along the sternum. The facts, as stated in a condensed form in the paper above referred to, led to the inference that the case was "one of aortic and mitral disease in combination, and possibly, also, of disease of the valves of the right side." The state of the heart, revealed by post-mortem examination, was briefly as follows:—"Mitral orifice of size only sufficient to admit the thumb, and the edges of the valve roughened with vegetations; similar vegetations on the free edges of the aortic valves, which were somewhat deformed and incompetent; tricuspid orifice just admitting two fingers, and evidently contracted, but without much shortening of the chordæ tendinæ, or roughness of the edges of the opening; pulmonic valves normal." See *Medical Times and Gazette*, as above, p. 462.

the writings of some authorities. I have seen numbers of them every year since I began hospital practice, and you cannot possibly fail to have many opportunities of observing them, although, perhaps, few cases are so perfectly clear and uncomplicated as that of Mary O'M., and even the case of J. M'L., now under notice. The more common form of disease is perhaps the combination, in about equal proportion, of mitral obstruction and regurgitation, each giving rise to its own characteristic murmur."

ART. 67.—*On Direct Mitral Murmur.*

By Dr. WILKS, Assistant-Physician to Guy's Hospital, &c.

(*Medical Times and Gazette*, Feb. 25, 1865.)

Dr. Wilks supplies the particulars of a post-mortem examination, and makes some comments upon them, which are of special interest when taken in connexion with the preceding article by Dr. Gairdner. "The body," says Dr. Wilks, "presented most of the well-known appearances of those who have died of heart disease. The lungs were apoplectic; the liver in the condition known as nutmeg, &c. The heart exhibited in a marked degree the characteristics of obstructive mitral disease; the left auriculo-ventricular orifice was so narrow that it would only just admit the point of the finger. This was due to the excessive thickening of the curtains and cords of the valve; the right side of the heart was somewhat enlarged; the left ventricle was about its usual size, whilst the left auricle was most enormously dilated, and at the same time its walls very tough and much thickened; the size of this cavity was even greater than appeared when the heart was removed, since the pulmonary veins were much increased in size and added to the capacity of the auricle. It contained a clot which had evidently been forming for some days prior to death."

Dr. Wilks then remarks:—"This case is of the greatest possible interest in connexion with the diagnosis of a contracted mitral orifice, and proves as nearly to demonstration as circumstances will permit the existence of a bruit produced by the blood passing through the narrow channel from the auricle to the ventricle, or, as it is called, a direct mitral murmur. In looking at this heart, you will see that every circumstance is present which is requisite for the production of such a sound. We know that fluid passing through a narrow channel is productive of a murmur as in the case of a constricted aortic orifice, and therefore you might feel some surprise at finding that many of the highest authorities have disputed and still dispute the possibility of a murmur occurring under the same circumstances at the mitral orifice. When a mitral valve is defective, it is admitted that a sound may be produced by the regurgitation of blood through it, but in many cases where a sound supposed to be dependent on such a cause has existed, there is no proof that the valve has allowed such a flow of blood backwards; in fact, in very many cases, of which

this is an extreme instance, the appearance of the heart would prove the contrary, and thus you should always attentively examine the condition of the cavities in order to form a conclusion as to the interrupted circulation through them. In this instance, the small size of the left ventricle was a sufficient proof that a small amount of blood only had been sent into it, and the whole of this had been transmitted onwards; whereas the immense size of the auricle showed at once the obstruction which had existed to its discharge of blood, just as the bladder is seen to be hypertrophied in stricture of the urethra. Here, then, is clearly a case showing, without any possibility of mistake, that the blood had been driven through a narrow mitral chink with considerable force; also, it is tolerably certain that there was no regurgitation, for the edges of the valve closed well, and the heart was not enlarged in a manner to produce theoretically a pericardial sound. If, then, a bruit was heard, there could scarcely be a doubt that it was produced in the manner suggested by the passage of blood from the auricle to the ventricle, or that it was a direct mitral or systolic auricular murmur. Why, then, has this not been admitted by the highest authorities on cardiac disease? For the explanation we are indebted especially to Dr. Gairdner, who has put the matter in a clear light, and shown that we do find a murmur where we should expect one under such circumstances as exist in the present case. The explanation depends upon the more correct method of regarding the heart's action. It had hitherto been supposed that the auricles and ventricles opened and closed alternately, and that it was during the contraction of the aorta and the second sound of the heart that the auricle contracted; consequently it was thought that if a direct mitral occurred it would be heard at the time of the diastolic sound and at the apex of the heart. It was denied that a diastolic mitral was ever heard in this position, and, therefore, that a sound was not produced by the transmission of blood through the auriculo-ventricular orifice. Dr. Gairdner, however, points out that the auricle contracts after the second sound and immediately before the first sound; and, therefore, a direct mitral murmur should be heard immediately preceding the impulse of the heart; that it is presystolic, although previously mistaken for a systolic. If this is proved, it would place the subject at rest. Now, this was done in the present case by Dr. Gull, who distinctly and repeatedly stated to his class that the bruit was presystolic, and that in consequence the mitral orifice was narrowed. Taking, therefore, the positive diagnosis made during life by Dr. Gull and the condition of the heart found after death, no reasonable doubt can remain that in this instance the bruit heard was produced in the manner named."

ART. 68.—On the Condition of the Heart in Emphysema of the Lungs.

By Dr. WATERS, Physician to the Liverpool Northern Hospital.

(*Lancet*, Nov. 12, 1864.)

Amongst the more prominent symptoms of general or lobar emphysema of the lungs is an altered condition of the heart. The expansion of the lungs pushes the cardiac organ downwards, backwards, and towards the mesial line; so that in advanced states of the disease the heart is felt and seen to beat in the epigastrium beneath the ensiform cartilage: the cardiac region becomes resonant from the overlapping lung, and the lower site at which the sounds of the heart are best heard is altered. These are facts familiar to all who have given attention to pulmonary diseases, and it is not to them, but to certain changes which take place in the walls of the heart, that Dr. Waters wishes to direct attention.

It has been the opinion of many pathologists that the right cavities of the heart alone become affected in emphysema; but the researches of Gairdner, Lebert, and others have satisfactorily proved that, in the majority of cases, where emphysema is extensive and of long standing, the cardiac disease is not confined to one side. Dr. Waters' own observations tend to confirm this opinion, and to show that the form of heart-disease most frequently associated with emphysema is a general hypertrophy and dilatation of the ventricles; for he has never seen a post-mortem examination of a case of extensive and long-standing lobar emphysema in which the left ventricle, as well as the right, was not affected.

That this diseased condition of the heart is a consequence of emphysema we can have no difficulty in believing when we consider the nature of the lung-affection, and especially that the palpitation, and other cardiac symptoms, are always preceded by dyspnoea and other signs of the pulmonary malady.

But hypertrophy of the ventricles is not the only change which takes place in the heart, for valvular disease is frequently found. The deposits which occur about the valves are no doubt secondary to the changes which take place in the muscular walls, and must be attributed to the general mal-nutrition produced by the pulmonary disease.

Can we give any satisfactory explanation of the causes which lead to hypertrophy and dilatation of the cavities of the heart in emphysema?

When we consider the anatomical arrangement of the pulmonary tissue, and especially of the pulmonary bloodvessels in the disease, we can have no doubt as to the manner in which the right side of the heart becomes influenced. The impediment which exists to the circulation through the lungs, in consequence of the physical condition of the lung-tissue, and the imperfect aëration of the blood, together with the diminution in the number of the pulmonary blood-

vessels, must necessarily give rise to an overloaded state of the right cardiac cavities, and to increased action on their part.

No such explanation as that just given will, however, apply to the hypertrophy, so commonly found, of the left ventricle; for there is a diminution in the quantity of blood which finds its way into that cavity, and consequently, on this account, rather less call for muscular action than in a state of health. We might therefore infer that we have in these circumstances an element of atrophy, rather than of hypertrophy. But morbid anatomy teaches us that the latter usually exists. Whence, then, does it arise?

It appears that we must look, in great measure, for an explanation of the fact to the effect produced on the heart by the displacement it undergoes in the disease. This displacement is always the greatest where the emphysema is most extensive, and it is in such cases that the left ventricle becomes most hypertrophied. As the lungs expand, the heart is pushed away from its normal position; and consequently, the direction of the axis of its cavities is altered with reference to that of the vessels connected with them. The ventricles of the heart are so placed, in a state of health, with regard to the arteries which issue from them, that no impediment exists to the onward passage of the blood, and the circulation is effected with the smallest possible expenditure of muscular force; but displacement of the heart necessarily alters the relations between these several parts, and produces an embarrassment of the heart's action—an embarrassment that can only be overcome by more powerful contraction. We consequently find that hypertrophy follows.

This appears to be the chief reason that must be assigned for the occurrence of hypertrophy of the left ventricle in emphysema. It is true that, in consequence of the condition of the venous system, some increased force may be required on the part of the left ventricle, and thus another element for the production of hypertrophy may exist. The altered situation of the heart is, however, I believe, the main cause; and this must have an influence in producing the changes which take place in the right ventricle, but here another cause is also at work.

Amongst the symptoms of emphysema, resulting from the changes which have just been referred to, must be enumerated the powerful impulse of the heart, often felt in the epigastric region, together with the smallness and feeble character of the pulse, as felt, for instance, at the wrist. A knowledge of the condition of the heart and of the state of the pulmonary circulation serves to explain these phenomena. The powerful impulse of the heart is the result of its hypertrophy, and embarrassment from position; whilst the smallness of the pulse is due to the small quantity of blood which the left ventricle expels at each beat, and its feebleness to the fact that the force of the left ventricle is expended, in part, in overcoming the resistance which exists to the passage of the blood from the ventricle, in consequence of the altered position of the latter, and in part merely in distending the arterial tubes. The diminished circulation through the pulmonary tissue, and the accumulation of the blood in the venous portion of the circulating system, sufficiently account for

the small quantity of that fluid which the left ventricle has to react on at each beat.

ART. 69.—*On Smoking as a Cause of Fatty Heart.*

By Dr. HENRY KENNEDY.

(*Dublin Medical Press*, April 20, 1864.)

Dr. Henry Kennedy, in a paper read before the Surgical Society of Ireland, on fatty heart, makes the following observations on the influence of tobacco-smoking in its production:—

"I must notice one [cause of this disease] which has year after year been gradually forcing itself on my attention, till it has now reached the strongest conviction in my mind—I mean the habit of smoking, which, I believe, I have traced in many instances to have been the predisposing cause of the disease. No one is more aware than myself of the difficulties which beset a question of this sort, nor the great opposition which, for obvious reasons, it is likely to meet. Still, the opinion has not been taken up hastily, nor, as I think, without such proof as the subject admits of. All will recollect that within a very few years a great paper war was carried on in the pages of the *Lancet* on the effects of tobacco, and the opinions expressed were sufficiently contradictory. Amongst them all, however, I did not observe one point noticed which seems to my mind of great importance in this question. It is the fact that if anyone, no matter what his temperament may be, gets out of health, so that the powers of his system are lowered, he must either lessen his smoking or give it up entirely. I have met no exception to this statement, which every one may test himself—as, for instance, in cases of paralysis, no matter how slight they may be. From the fact, however, I conclude that tobacco, besides other effects, is a depressor of the nervous system, and that there is a constant antagonism going on between it and the healthy state of the constitution, and when used too freely it ultimately engenders a state of health which is very apt to be followed by a fatty heart. At any rate, whatever the explanation may be, the fact is as stated above, and I have seen now too many cases of fatty heart, in what are called heavy smokers, to have any doubt on the matter.

"This day, 4th March, a case which strongly confirms some of the remarks just made came under my notice, and for the third time. The patient, aged thirty-four, is a man of full height, made in the very finest proportions, and remarkable, or at least was, for great physical strength and activity. He has always been strictly temperate as regards strong drink, but is the heaviest smoker I recollect to have met. About three months since he began, and without any cause he could discover, to lose flesh and strength very rapidly, and his wind, as he called it, became so short that he was obliged to give up active exercise. He now looked pale and depressed, having had a cold

which he found it hard to shake off. He told me he had, at my wish, twice tried active exercise since I last saw him. On the first trial he got through it but badly; on the second he was forced to give it up, as his breathing became so hurried and his heart beat so violently. It seems scarcely necessary to add that he had been driven to give up his darling tobacco.

"Except the pulse, there is nothing in this case to indicate disease. The two sounds of the heart are distinct and unattended by murmur. There is no increase of dull sound on percussion, nor can I say that the impulse varies from health. Whilst he sits, however, the pulse beats but forty-eight in the minute, and it was just the same from the first time I saw him. It is large and full to the finger, under which it passes slowly, and is readily compressed. Any movement at once increases the beats, and more than occurs in the healthy state.

"Now, in this case I have scarcely a doubt that the heart has become fatty, and most probably in the worst form: I mean where the muscle itself has degenerated. Yet, he tells me, he passed a physician and had his life insured just five months since!"

ART. 70.—*Chordæ Tendineæ of Diseased Mitral Valve ruptured during Violent Vomiting.*

By Dr. PEACOCK, Physician to St. Thomas's Hospital, &c.

(*Medical Times and Gazette*, Jan. 7, 1865.)

At one of the meetings of the Pathological Society held at the close of last year, Dr. Peacock exhibited the heart of a female aged twenty-one, who died recently at the Victoria Park Hospital. She had been an in-patient of the Hospital in 1851, labouring under cardiac symptoms, the sequence of an attack of rheumatic fever two years before. She had not been under any medical care for some time, and was in her usual health, when she was taken in a cab from her home in Artillery-street, Bishopsgate-street, to the Victoria Park Hospital to be examined for admission. The motion of the cab was very uneasy, and shortly after leaving home she vomited violently; she was then attacked with difficulty of breathing and faintness, and when she reached the Hospital, a distance of scarcely a mile and a-half, she was pulseless at the wrist, and apparently dying. When seen by Dr. Peacock, a few minutes after arrival, she had in some degree rallied; the pulse was feebly to be felt at wrist, but she was too weak to be carefully examined, and the loud tracheal rhonchus rendered it difficult to hear the cardiac sounds. It was, however, thought that there was a feeble systolic murmur at the apex of the heart. She was removed to bed, but died in a few hours. On examination after death the pericardium was found to be entirely attached by old and firm cellular adhesions. The heart when deprived of the adherent pericardium and freed from coagula, weighed $20\frac{1}{4}$ oz. av. (the healthy range for males being 9

to 11 oz.) There was considerable dilatation of the left ventricle, but the walls were not increased in thickness, measuring from .1776 to .3996 of an English inch (the healthy average in males being .541 of an English inch). The aortic aperture was somewhat contracted, measuring 2.93 of an English inch (average in males 3.145), and the ventricular surfaces of the valves were considerably thickened, while the crescentic edges were free. The left auriculo-ventricular aperture was slightly diminished in capacity, admitting only a ball .3996 of an English inch in circumference (the healthy average in males being .402 of an English inch). The mitral valve was considerably thickened and indurated, and the chordæ tendinæ of the free fold of the posterior columna carnea were entirely torn from their attachments, so that the valve was entirely loose, and must have admitted of free regurgitation. The walls of the right ventricle were somewhat thickened, measuring .1776 of an English inch (healthy average .1666 of an English inch). The right auriculo-ventricular aperture was slightly constricted, measuring .3996 of an English inch (healthy average .4791 of an English inch), and the tricuspid valves were thickened and firmer than natural. The lungs were much congested, and the bronchi contained bloody mucus.

Dr. Peacock remarked there could be little doubt that the tendinous cords had been torn from their attachments during the violent vomiting soon after the girl entered the cab, for her mother expressly stated that she did not suffer at all seriously when she left home, and that she had not the slightest idea that there was any risk in taking her to the Hospital. Though cases of rupture of valves or their attachments, when diseased, were of more common occurrence than such injuries to healthy valves, yet all such cases were very rare. The case did not throw light upon the question of the effect of universal adhesion of the pericardium to the heart upon the functions and nutrition of that organ, for there was considerable disease, both of the aortic and mitral valves, which would explain the great increase of weight. He had, however, seen specimens in which the pericardium had been universally attached, evidently from inflammation of old date, in which the size and weight of the heart did not exceed the healthy average.

ART. 71.—*Rupture of the Heart.*

By Dr. MÜLLER.

(*Würtemb. Corr. Blatt*, 1864; and *Schmidt's Jahrbücher*, No 4, 1865.)

CASE.—The patient was a woman, sixty-two years old, who often complained of pain at the heart, and who died suddenly. The pericardium was distended with blood, partly fluid, partly coagulated, the right ventricle was dilated, and presented a laceration an inch and a quarter in length. The tissues around the rent were softened, thin, and friable; and the whole heart was flabby.

ART. 72.—*Laceration of the Aorta.*

By Dr. HEADLOFT.

(Preuss. Ver. Ztg., 1864; and Schmidt's Jahrbücher, No. 5, 1865.)

The author records two cases of this accident:—

CASE 1.—In the first, the patient was a man, twenty-nine years old, of weak constitution, but previously free from disturbance of the breathing or circulation. He was attacked by gastric pain and vomiting. Six days later, after a fresh attack of pain, he died. There was found a rent, an inch and a half in length, on the inner side of the arch of the aorta.

CASE 2.—The second patient died soon after an attack of sudden pain, having previously been out mountain climbing with an excursion party. The author believes the rupture to have existed, in each case, for some time before death; in the first, from the attack of vomiting—in the second, from the climbing.

ART. 73.—*On the Treatment of Aneurism by Lead.*

By Dr. G. OWEN REES, Physician to Guy's Hospital.

(Lancet, March 18, 1865.)

The following case may perhaps be read with interest, as illustrative of the treatment of aneurism on a new plan—viz., that of introducing lead into the system, as a remedy possessing the power of facilitating the coagulation of the blood, a full diet being given at the same time. Dr. Owen Rees' chief object in publishing the case is to induce others to treat aneurism in like manner, in order that the fact here recorded may assume its proper value, and be regarded either as a mere accident or as an effect of the treatment employed.

CASE.—W. F.—, aged twenty-seven, residing at Greenwich, was admitted into Stephen ward, Guy's Hospital, on the 26th of October, 1864, under the care of Dr. Owen Rees. He was the subject of popliteal aneurism, and had been taken in by Mr. Poland, who transferred the case in order that Dr. Rees might have an opportunity of making a therapeutical experiment. The patient states that seventeen days ago he was out walking, when, on standing still, he felt pain under the left knee. The pain was relieved by flexing the knee-joint. He then observed a tumour over the seat of pain, which has increased in size up to the present date, but has never caused him much trouble. He has ulcers and varicose veins on both legs, the left being the worse. He is a strong, healthy-looking man, single, and a teetotaler, and has always enjoyed good health. Has worked very hard lately. The heart-sounds may be considered normal, if we except a slight prolongation of the second sound. The bowels are generally constive. A distinct bruit is heard over the seat of the aneurism, and pulsation is well marked. Mr. Poland's description of the tumour is as follows:—"The aneurism was of the size of a duck's egg, and its contents were perfectly fluid. The walls were excessively thin, so that it was feared rupture would take place. Arrangements were made to commence compression at once, and a failure

ensued. Deligation of the artery to be performed. Pressure on the artery above readily commanded pulsation, and was attended by complete emptying of the sac without apparent trace of coagulum. It was considered that the opening of the artery into the sac was of large diameter."

Oct. 29th.—Ordered three grains of acetate of lead and one grain of opium powder three times a day; to have full diet, and a chop for breakfast.

Nov. 1st.—A slight blue line on the gums.

4th.—Complains of loss of appetite, and has frequent calls to stool; the bowels, however, do not act.

5th.—Ordered five grains of acetate of lead and one grain of opium powder three times a day.

18th.—He complains of loss of appetite. His condition is much the same as on the 4th. Ordered an ounce of castor-oil immediately.

24th.—The swelling in the popliteal space has been gradually hardening and enlarging, apparently owing to the deposit of fibrin within the sac of the aneurism.

26th.—Ordered two drachms of castor-oil immediately.

Dec. 2nd.—Is obliged to discontinue taking the pills on account of the colic produced. The pulsation in the tumour is very much less.

3rd.—Feels easier; has less pain in stomach. Ordered the magnesia and salt mixture twice a day.

5th.—Slept badly, having pain in his stomach; his appetite has much diminished.

12th.—The pulsation has ceased, but the tumour is larger and more tender, and he cannot straighten his leg on account of the mechanical obstruction caused by it.

15th.—Mr. Hilton found that by pressing the femoral artery he could affect the sac, but thought the aneurism was almost cured. The patient has now a clear, well-defined blue line on the margins of the gums, and feels no pain in the stomach. The articular arteries of the left knee-joint can be seen pulsating very distinctly.

17th.—The tumour, which almost fills the popliteal space, is diminishing in size. He can nearly straighten his leg. The left leg is flabby, and swells if he stands much upon it, and it measures two inches more in circumference than the right.

20th.—The tumour is fast diminishing in size.

31st.—He has left the hospital to return to his work.

On Jan. 17th he came to the hospital to be examined by the surgeons, and was seen by nearly the whole surgical staff, amongst the rest by Messrs. Cock, Hilton, and Birkett. He was considered to be perfectly cured. He states that he cannot walk far without feeling a numb sensation over the calf of his left leg, circulation by anastomosing branches being not yet freely established. He has been able to do his work. There is still a clear, well-defined blue line on the margin of the lower gum; the upper is less affected. There is also some enlargement in the popliteal space, which he feels somewhat inconveniently when walking.

"It will be observed," says Dr. Rees, "that the doses of lead were large and continued. Thus three grains of the acetate were given three times a day for six days, and then an increase was made to five grains, a grain of opium being given with each dose. This five-grain dose was continued for twenty-six days. With regard to the pains in the abdomen, they never amounted to colic, and my surprise is that the patient did not suffer more. His attention was directed to the probable occurrence of such pains, and latterly he

was inclined to exaggerate them. Though rest was enjoined, the patient would not submit to it. He walked about the ward as it pleased him, and, I am informed, danced on one occasion for the amusement of his companions. It is worthy of remark that when the lead had been taken for three weeks the aneurism had hardened very obviously, and my belief is that we might be content to discontinue the lead should an aneurism begin to change as above described. The system is charged with the metal, and deposit once begun, we may fairly expect it will continue to the filling up of the sac.

"It may be well to mention that the only other case of aneurism in which I have exhibited lead was in that of a man the subject of so advanced a stage of thoracic aneurism that spinal absorption had occurred and paraplegia had been produced. The tumour was clearly visible on the left side of the spine. The case was obviously hopeless, but I gave the lead, in order, if possible, to ascertain what effect it might produce on the structure of the fibrin in the sac on post-mortem examination. The man, however, left the hospital, and, I hear, died shortly afterwards."

(D) CONCERNING THE ALIMENTARY SYSTEM.

ART. 74.—*Epidemic of Mumps at Breslau.*

By Dr. JOSEPH.

(*Berlin. Klin. Wochenschr.*, No. 30, 1864.)

Dr. Joseph records an epidemic of mumps at Breslau, which occurred during the first three months of 1864, and chiefly affected young people from six to nineteen, with no decided preference for any particular age between these. Dr. Joseph himself saw thirty-four cases, of which twenty-five were in one house, an institution for deaf mutes. Neither in the deaf mutes nor in other patients was there any apparent connexion between the state of the ears and the disease; and the majority of the persons attacked were previously in good health. The parotitis usually commenced on the left side; and in nearly a third of the cases was confined to that side; but in the majority the right was also affected a few days later. In two cases the disease commenced on the right; and in one of these did not extend to the left.

The attack commenced with slight febrile symptoms, and pain in the parotid gland on crowing or pressure, before the appearance of swelling. The opening of the parotid duct was constantly examined, and never differed in appearance from the surrounding mucous membrane. The cases were mild, terminating in recovery between the 7th and 11th day after the first symptoms; but were more protracted in three feeble children. No sequelæ of any kind were observed, and no metastasis to the testicles or elsewhere.

The seat of the disease appeared to be in the interlobular texture of the gland. A examination of the saliva in four cases showed a

normal reaction while fasting, an alkaline reaction while chewing. Sp. gr. 1.005—1.006, and the power of saccharine conversion somewhat diminished.

The observations of Dr. Joseph led him to believe the disease decidedly contagious; but that the *materies morbi* was not excreted through Steno's duct.

The treatment was limited to the use of mineral acids and gentle laxatives, and the local application of mercurial ointment with belladonna, and in two cases, of ointment of iodide of potassium.

ART. 75.—*Paralysis of the Palate after Simple Angina.*

By Dr. DE LUCÉ.

(*Bull. de Thé.*, Juin 30, 1864.)

Dr. de Lucé reports three cases of paralysis of the palate following simple angina. In all, cauterisation had been freely practised; and, in all, the paralysis was limited to the side originally affected. Dr. de Lucé regarded the paralysis as being purely local, and a result of the cauterisation.

ART. 76.—*Case of Ichthyosis of the Tongue.*

By Mr. J. W. HULKE, Assistant-Surgeon to the Middlesex and Royal London Ophthalmic Hospitals.

(*Proceedings of the Royal Medical Chirurgical Society*, Feb. 28, 1865.)

Ichthyosis is a term provisionally applied by the author to an affection of the mucous membrane of the tongue, which consists in hypertrophy of its epithelial and papillary tissues. It is characterised by yellowish white, raised, tough, leathery patches, which are clinically distinguishable from syphilitic nodes, condylomata, and cancer. Mr. Hulke has seen but one case, and this he relates.

This case was commented upon by several Fellows of the Society, and their comments are well worth remembering.

Mr. Birkett, in 1853, saw a patient, an old sailor, who had a similar affection to that described by Mr. Hulke. It had existed many years, and was merely an annoyance.

Mr. Holmes Coote remarked that he had published a similar case in Holmes's "*System of Surgery*," but had not given a name to the affection. It occurred in an old woman. There was no evidence of syphilis; it was not cancerous, but merely a thickening of the epidermis.

Dr. Stewart said that the year before last a gentleman from India came to ask him if an affection of his tongue was syphilitic or not. After due inquiries, there was no ground for the slightest suspicion of that as a cause. He had had syphilis four years ago, but had got well of it, and then married. Mr. Henry Lee saw him, and thought he was free from syphilis. All sorts of detergents were applied,

and the solution of the chloride of zinc had no effect. Dr. Neligan had described a similar condition of the tongue in a case which, after slight irritation, ended in cancer. His (Dr. Stewart's) patient was still in good general health.

Mr. Paget said the affection described by the author was sufficiently rare to warrant each member giving his individual experience. He referred to a specimen in the museum of St. Bartholomew's Hospital taken from a patient who died of cancer of the tongue, with disease of the lymphatic glands. Although ultimately the disease was cancerous, it had for ten years resembled the condition described by the author of the paper. This patient also used to pare down the growth as if it were a corn. He (Mr. Paget) did not wish to imply that the form of disease was more likely to end in cancer when this patient became old than many other diseases of the tongue which were accompanied by irritation. Mr. Hulke's case was the first of the kind placed on record, and was interesting as suggesting that diseases of the tongue should be studied in connexion with those of the skin. Mr. Paget then related a case which was, he believed, an instance of ringworm on the tongue.

Mr. C. H. Moore alluded to a case in which it was difficult to distinguish cancer of the tongue from other affections of that organ. He referred to a case then under his care in Middlesex Hospital, in which there was an ulcer of the tongue, and opposite the growth was a patch of disease like that described by the author. There was also a similar patch on the cheek. Although it was clear that Mr. Hulke's case was not yet one of cancer, yet it would be a bold thing to assert that cancer would never occur in the part diseased. Although at present only an hypertrophy, it might afterwards become cancerous.

ART. 77.—*On Gastric Ulcer.*

By Dr. MIGUEL.

(*Hannöv. Zeitscht. f. prakt. Heilkund*, I. 1864; and *Schmidt's Jahrbücher*, No. 1, 1865.)

Dr. Miguel contributes an elaborate memoir upon gastric ulcer, embracing symptomatology, etiology and treatment. Under etiology, he says:—

Gastric ulcers, either open or cicatrized, are found in from two to four-eight per cent. of all bodies examined after death; the cicatrized having a great numerical preponderance. Ulcers are three times as frequent in women as men, and their course in men is less favourable. In women, they occur mostly between fifteen and thirty, and are scarcely ever met with before the first-named age; in men, they occur at all periods of life with equal frequency. This statement is based on the statistics of the Prague Hospital, recorded by Willigk and Jaksch; and the author says that Brinton's statistics are inaccurate.

The author attributes the comparative frequency of the gastric ulcer in women, as well as the limitations of age, within which it is

found, almost entirely to the influence of local congestions dependent upon menstrual irregularities. He also believes that the weak or cachectic condition of many of the sufferers is a consequence, rather than a cause, of the affection.

With regard to treatment, the author assumes, from the frequency of healed ulcers, that a favourable prognosis may generally be formed; although it must be guarded in proportion to the certainty of the diagnosis, and to the presence of fixed pain or hæmorrhage, both of which are indications that the ulcer has already passed the limit of the mucous membrane. The chief indication is to afford as much rest as possible to the stomach, and for this purpose to plan the diet with a view to avoid both chemical and mechanical irritation. Food should be light, digestible, and given only in small quantities at once, so as neither to excite any copious secretion of gastric juice, nor to distend the stomach, nor to produce energetic digestive movements. The various kinds of milk food, combined with antacids, will be found most desirable. Where a more stimulating diet is required, meat soups, even tender meat itself, wine, and beer, may be given. Hunger often increases pain, and taking food relieves it. Only moderate exercise should be permitted; strong exertion being hurtful by the absolute movements, as well as by causing an increased demand for food. The author treats also of the treatment of special symptoms, pain, spasm, vomiting, and so forth; but without suggesting anything that requires to be recorded.

ART. 78.—*Upon Syphilis of the Liver.*

By Professor OPFOLZER.

(*Wien. Med.-Halle*, IV., 10, 24, 26, & 27, 1864; and *Schm. Jahrb.*, No. 11, 1864.)

Syphilitic disease of the liver may exist:—1, as simple peri-hepatitis; 2, as interstitial hepatitis; 3, as gummy hepatitis; and, 4, as colloid or amyloid degeneration. These varieties may be present either separately or in combination, but the latter is most common. The peri-hepatitis is very seldom seen alone, and is most frequently complicated with the interstitial form; while the gummy hepatitis is often associated with the amyloid or fatty liver.

The peri-hepatitis, which, as before stated, is commonly associated with interstitial inflammation, produces a hard, tough thickening of the capsule of the liver. Externally, this diseased capsule becomes adherent to neighbouring organs, as the diaphragm, and, less frequently, to the stomach and colon: internally, it encroaches upon the parenchyma, especially on the convex surface, and gives to the liver an irregularly lobular appearance. An incision displays a tendinous vascular fibrous tissue, which penetrates more or less deeply into the usually atrophied glandular tissue. This condition may be confined to a single lobe, or may affect the entire organ.

In the gummy form of hepatitis, we find (in a liver that may be otherwise normal, or the seat of colloid degeneration, or presenting

the capsular change above described) nodules ranging from the size of a bean to that of a walnut, and consisting of whitish or yellowish cheesy matter, sometimes converted into a cretaceous pulp or a calcareous mass. The substance of these nodules is usually penetrated by cords of fibrous tissue, although such "gummata" may occur without them, and in the midst of normal parenchyma.

The larger portal branches, as well as those of the hepatic artery and duct, often, but not always, escape. The degree in which they may be involved will determine the character of other symptoms, such as ascites, jaundice, and so forth. The tissue between the nodules may be either normal, or it may be hypertrophied, with enlargement of the acini and cells, so as to compensate for the loss of substance (Virchow), or it may be the seat of fatty or amyloid degeneration.

The whole volume of the liver is commonly somewhat increased in simple peri-hepatitis, much diminished (syphilitic cirrhosis) when the inflammation has attacked the interstitial connecting tissue. When gummata are present, there is usually moderate enlargement; although diminution may occur from wasting of the parenchyma. The syphilitic liver is constantly much enlarged, only when it is the seat of fatty degeneration.

Syphilis of the liver, not only possesses no special characteristic, but presents no single symptom that is even tolerably decisive; and the most careful and exact examination cannot exclude errors of diagnosis. The sensations of pain or weight, of which patients usually complain, are present, more or less, in nearly all hepatic diseases. The same statement applies to jaundice. Ascites, and enlargement of the spleen, are even of less value; and a history of the case is the only guide to a correct conclusion. The practitioner will also be assisted by a close examination of the parts and organs in which syphilis is commonly manifested.

It is scarcely possible to determine to what stage of syphilis the liver affection should be assigned. Oppolzer, Dettrick, and Gubler, have seen instances in which hepatic disease appeared to rank among secondary phenomena, but most of the cases belong to the tertiary group. Disease of the liver is also frequent in children with inherited syphilis.

The prognosis is not so unfavourable as many writers have assumed. We may, sometimes, find syphilitic changes in the livers of patients who have died from other causes, and who had displayed no marked hepatic symptoms. As a rule, the prognosis is more favourable when there is hypertrophy, than when atrophy has commenced. The occurrence of cachexia, dropsy, obstinate diarrhoea, or degeneration of the kidneys, must be regarded as very unfavourable.

The treatment is that of constitutional syphilis. Iodide of potassium, mercurial fumigations, tonics, and appropriate regimen, if they fail to cure the disease, may, at least, arrest its progress.

ART. 79.—*Syphiloma of the Liver.*

By Dr. E. WAGNER.

(Arch. d. Heilk., V., p. 121, 1864.)

Dr. Wagner's object is chiefly to maintain, 1, that both the nodular and the diffused syphiloma of the liver are identical with syphiloma of other organs; and, 2, that the interstitial syphilitic inflammation of the liver is the result of a kind of healing process of the circumscribed syphiloma. The original paper contains a large number of cases, and also a good description of congenital hepatic syphiloma.

(R) CONCERNING THE GENITO-URINARY SYSTEM.

ART. 80.—*On the Treatment of Diabetes.*

By Dr. H. BENCE JONES.

(Medical Times and Gazette, Jan. 20 and Feb. 4, 1865.)

Almost every substance in every Pharmacopœia has been tried as a specific for diabetes; but hitherto no remedy has been found to have a constant effect in stopping the sugar from appearing in the urine.

This by no means renders it impossible or improbable that some substance may be discovered which may be able to effect a perfect cure of this functional disorder.

Meanwhile, the effect of diet is far beyond that of any known remedy.

An anti-farinaceous, or, in other words, an anti-saccharine diet will remove the sugar from the urine, and stop all the symptoms of the complaint in all those cases in which the power of consuming the animal sugar remains unaffected.

Even when the consumption of the animal sugar is imperfect or impossible, an antisaccharine diet will lessen the thirst, the flow of water, the dryness of the mouth, and even the constipation, and check, though it may not stop, the waste.

The simplest formula for the diet may be thus stated. All animal produce, including fish, flesh, fowl, game, eggs, cream, and meat soup should be taken; and all vegetable food that contains starch, dextrin, and sugar should be avoided.

As generally it is of the utmost importance to shun the forbidden food, the author dwells upon it first.

The vegetable substances that contain most starch, dextrin, and sugar are rice, maize, arrowroot, sago, potatoes, oatmeal, peas, beans, bread, biscuit, toast, macaroni, vermicelli, and all confectionery.

Fruits are even worse than vegetables. Apricots, plums, peaches, cherries, pears, gooseberries, are nearly as bad, and some worse, than rice and maize. Stout, porter, and ale, cider, port, Madeira, champagne, and sherry are more or less highly saccharine; cocoa and

chocolate contain near twenty per cent. of starch and dextrin naturally, and more is often added.

The harm of each of these substances may be determined by the amount of starch, dextrin, and sugar they contain.

The following table, in which the fruits and farinaceous vegetables are taken as perfectly dry, will answer many questions regarding the diet of a diabetic patient:—

	Amount of Starch, Dextrine, or Sugar.	
Ripe dry Apricots	about 93 per cent.	
„ Plums	92	„
„ Peaches	86	„
„ Cherries	85	„
„ Pears	84	„
„ Figs	79	„
„ Gooseberries	37	„
Dry Rice	90	„
„ Maize	88	„
„ Arrowroot	77	„
„ Potatoes	76	„
„ Oatmeal	70	„
„ Peas	67	„
„ Beans	67	„
„ Bread	61	„
„ Milk	21	„
Stout	about 45 to 64 grs. of sugar per ounce.	
Porter	23	40 „
Ale	12	45 „
Sweet Cider	18	44 „
Port	16	34 „
Madeira	6	66 „
Champagne	6	28 „
Sherry	0	12 „

If dry rice contains 90 per cent. of starch, dextrin, and sugar, and potatoes contain 76 per cent. of starch and dextrin, and if all the starch and dextrin pass off in the urine as sugar, it is evident that to forbid potatoes and to allow instead an equal quantity of rice is simply ordering the quantity of sugar from this source in the urine to be increased 14 per cent., thus adding to the thirst and waste. Or, if half-a-pint of port wine is forbidden containing from 128 to 272 grs. of sugar, and if a pint of porter or stout is ordered which contains from 368 to 960 grs. of sugar, it is clear that the quantity of sugar in the urine will thus be increased from half-an-ounce to an ounce and a-half daily.

Before passing to the best diabetic diet, there are two substances—bread and milk—which require to be further mentioned here.

Ordinary bread contains water, salts, starch, dextrin, sugar, and gluten.

If the salts, starch, dextrin, and sugar are washed away, the gluten remains, which, in a chemical point of view, is as unobjectionable as meat.

In making the different kinds of gluten bread this washing is more or less perfectly performed.

In the following analysis the water, starch, dextrin, and sugar were determined, the residue or difference was taken as gluten :—

	Water.	Starch and Dextrine.	Sugar.	Gluten.
Ordinary bread	36	40	1	23 per cent.
Aërated bread	37	42	2	19 „
Gluten bread from				
Toulouse	2	16 to 44	0	82 to 54 „
Dried bread	2	60	1	37 „

The best washed gluten bread contains less starch than bran cakes or any brown bread; Dr. Pavey's almond bread is free from all starch, but the almond flour must be well washed to remove the sugar and dextrin, of which ten per cent. are present.

Badly-washed gluten may be made into dry bread containing bulk for bulk more starch than ordinary undried bread; thus an excess of gluten bread may keep up the amount of sugar in the urine, and prevent an improvement in the symptoms.

With regard to milk, one hundred parts may be taken to contain three parts of lactine, or about half-an-ounce of lactine exists in a pint of milk. If all of this animal sugar was incapable of being consumed in the system, milk would be nearly as injurious as an equal quantity of many wines, and the best sweet ale; but experiment shows that this sugar is often partly or entirely consumed.

A diabetic patient lived upon butchers' meat alone for two days. The quantity of urine passed was forty ounces the first, and forty-two ounces the second day; specific gravity, 1029.0. No trace of sugar could be found. He then took milk for two days; the first day eighty-eight ounces, the second day ninety-nine ounces. The urine was forty-five and a-half ounces the first day; specific gravity, 1024.1: and the second day sixty-nine ounces: specific gravity, 1011.9. Sixty-seven grains only of sugar were passed the first day, and twenty-three the second day.

At a more advanced period of the disease, when strictly animal diet did not cause the sugar to disappear from the urine, milk alone was again taken.

The first day 138 ounces of milk were drank; the urine was 61 ounces; specific gravity, 1030.5. The second day 88 ounces of milk were taken; the urine was 34½ ounces; specific gravity, 1027.8. The quantity of sugar in the urine was 854 grains the first day, and 414 grains the second day. At this time, on animal diet alone, one day 280 grains of sugar were passed, and the next day 600 grains.

Thus milk is more or less injurious according to the stage of the complaint. When animal sugar can be consumed, milk is comparatively harmless.

In the advanced stages of the complaint, curds of sour milk, with the acid nearly neutralized by potass, soda, or ammonia, would be as unobjectionable as gluten bread.

There are two great ends to be gained by the use of medicine in diabetes.

Of these, the first and most important is to promote the oxidation of the sugar; or, failing this, to compensate the system for the loss of saccharine fuel, and the consequent loss of power and nutrition, by promoting the supply and oxidation of the oleaginous fuel.

Of all the medicines that can be given for the promotion of the oxidation, whether of sugar or fat, in the body, iron and alkalies are the most energetic; and hence, beyond all other remedies, iron, or the ammonia-citrate of iron, with excess of ammonia, or with other alkalies, are the best medicines for diabetes. The iron may be given in potass, or Vichy, or in Fachingen water, and that preparation which confines the bowels least is most to be preferred. Hence the potassio-tartrate and Griffiths' mixture are often useful.

Alkalies without iron promote oxidation. This is very evident in the copper test for sugar. M. Miahle even has stated that alkalies are the specific for diabetes; without doubt they are of importance in promoting oxidation. Soda or potass may be given in the caustic state, or as carbonates. Carbonate of ammonia, in ten, fifteen, or even twenty-grain doses, thrice daily, in any gaseous mineral, lessens the thirst.

Long since, Professor Graham tried the phosphate of soda, with three equivalents of soda, on the ground that the blood required this substance, and could not get it in the animal food. Dr. Bence Jones has not found any great advantage from its use.

Vichy water, more particularly the Celestin, is recommended by M. Bouchardat. It contains between eighty and ninety grains of carbonated alkali and alkaline earths in a quart of water. The Hospital spring contains about ninety-six grains, the Grande Grille ninety-two, the Hauterive eighty-nine, and the Celestin eighty-five grains to a quart of water.

Carlsbad Sprudel water, and the more aperient Muhl spring, is highly praised by Dr. J. Seegen. It contains about one-fourth, or less, of the alkali of Vichy water, but half a drachm, or more, of sulphate of soda in each quart, gives an aperient action which the common salt of Vichy water rarely possesses. Marienbad Kreutzbrun water is twice as aperient, but rather less alkaline than Carlsbad Sprudel. Fachingen water has one-third or more of the alkaline power of Vichy; Seltzer water one-sixth.

Besides alkalies, some animal substances are thought to promote change in the sugar in diabetes. Of these, rennet and pepsine may be mentioned. In 1852, Dr. James Gray published some remarkable results in the *Edinburgh Monthly Medical Journal*, p. 396; but it is not yet proved that rennet is very useful in diabetes. It is an albuminous substance in a state of change, and, therefore, it exactly fulfils the conditions required in the undiscovered specific for diabetes. It should be well washed with water to remove the adhering sugar and dextrin, and it should be given on an empty stomach, so as to enable it, if possible, to act on the sugar in the blood rather than on the sugar in the food.

Pepsine is another albuminous substance in a state of change. Its action in the stomach is to help the solution of the albuminous

food; but when it passes into the blood, it might be the animal diastase that carries on the change in the sugar. With this idea it has been given in diabetes, but with no satisfactory result; although many patients have said they were better whilst taking this remedy, yet the author has never found the sugar diminish under its use. It usually helps the action of the bowels.

Lately oxygen gas has been tried by Dr. Richardson in diabetes. It would be splendidly simple if this were the specific for diabetes. This view implies that a deficiency of oxygen is the cause of diabetes, but the chemistry of the disease does not admit of this explanation. There must be a much more complex chemical error than a want of oxygen, and it is far more probable that no deficiency of oxygen exists, but that it does not act because the proper animal diastase is not present.

Vegetable and animal oils and fats constitute important remedies in diabetes. Of all these, cod-liver oil and cream are most frequently used. The following case may be taken as an instance of the amount of cod-liver oil that can be given:—

A man, aged 24 years, was admitted into St. George's Hospital, having lost two stone in weight during eight months. He passed seven quarts of urine daily. He remained under treatment for a month, during which time he was on animal diet and cod-liver oil. He began with half an ounce daily, and this was gradually increased up to eight ounces. The quantity of urine fell to two pints and a-half, specific gravity 1030, and he increased in weight from 8 st. 8 lb. to 9 st. 1 lb.

Cream may be given in any quantity until the tongue begins to be coated, then it soon disagrees, and the stomach refuses to take it, or rejects it when taken.

Fats, combined with alkalies, as soaps, are more ready to undergo oxidation than when the glycerine is unseparated. Pure glycerine is, however, often very useful as a substitute for sugar in tea and in other liquids.

To lessen the thirst and the craving for food, opium is very useful. The alkaloids of opium diffusing out of the blood in contact with the nerves of the bloodvessels cause contraction of the capillaries; this affects secretion, so that the quantity of urine, saliva, bile, and intestinal secretion is greatly diminished. The drain of urine, and the desire for food are thus checked, but the increased constipation and dryness of the mouth, almost, and in some instances quite, counterbalance the gain obtained by checking the flow of water. By the use of very small quantities of opium, as five or ten grains of Dover's powder, or five or ten drops of laudanum once or twice daily, the thirst and excessive flow of urine may be stopped, and the constipation may not be excessively increased.

The second great object in the treatment of diabetes is to remove the constipation.

The excessive determination of water to the kidneys causes increased dryness elsewhere, hence the mucous membrane and the skin are harsh and dry; probably also the production of acid being

greatly diminished by arrest of change in the sugar, the healthy secretion by the intestines and skin cannot take place.

Notwithstanding the amount of food eaten, the action of the bowels usually is very difficult. All saline aperients increase the thirst and pass off by the urine. Magnesia, from the absence of acidity, is usually inactive. Castor oil is by far the best aperient, when it does not nauseate, then capsules, containing castor oil, with minute quantities of croton oil, are most efficacious. Compound extract of colocynth with jalapine, scammony, or gamboge, or podophylline, will act when oil cannot be taken. Mercury, when soluble, combines with the albuminous matters with which it comes in contact, and sets up increased chemical action. The nutrition of the parts to which it is directly applied, or carried, by absorption and diffusion, is so altered, that increased action, inflammation, and ulceration are produced. This altered nutrition has no influence on the oxidation of the sugar or fat, and hence diabetes is not benefited by mercury. Calomel may be used as an aperient, but it has not any advantage over other chemical or mechanical irritants to the mucous membrane of the bowels, and promoting a more rapid change in the albuminous structures of the textures causes a feebleness of nutrition to take place.

In extreme cases, the constipation of the bowels becomes the most serious symptom. The chemical disease leads to a mechanical difficulty amounting almost to an obstruction of the bowels. Strong chemical irritants are required to excite the muscular action, sometimes even "croton oil" is necessary. When the bowels do act, the prostration of the strength is sometimes alarming; so little power seems to be set free in the body, that any extra expenditure seems to deprive the heart of the force necessary to carry on the circulation, in the same way as a fatiguing journey to London for advice, or extreme mental anxiety, will bring a prostration of strength from which, with difficulty, recovery takes place.

ART. 81.—*The Simultaneous Employment of Perchloride of Iron and Ergot of Rye in Albuminuria.*

By Drs. SOCQUET and CHATIN.

(*Medico-Chirurgical Review*, October, 1864.)

Dr. Socquet, in the first instance, and afterwards Dr. Chatin, both physicians of the Hôtel Dieu of Lyons, have employed the perchloride of iron and ergot of rye for the prevention of the loss of albumen in the urine, and the results they have obtained are deserving of notice. The cases observed were some men of bad constitutions, weakened by former unfavourable hygienic conditions, such as insufficient food, and dwelling in damp and badly ventilated localities. The dropsy, in all the cases, at first confined to the face, had successively attacked the limbs and peritoneum. The urine was pale and inodorous, and contained large quantities of albumen, and in one case microscopic examination revealed the presence of the

remains of renal epithelium. Immediately on their admission into the hospital these men were subjected to diaphoretics, alkaline diuretics, uva ursi, and digitalis, though without any good result; but at last they took the ergot of rye and perchloride of iron. These medicines were given in progressive doses, beginning with 20 drops of tincture of the perchloride and 50 centigrammes of ergot of rye. Every two or three days these doses were methodically increased, and carried successively to 30, 40, 50, 60, 70 drops of tincture of the perchloride, and to 75 centigrammes, 1 gramme (about 15 grains), and three grammes of the ergot. Under this treatment the albumen in the urine rapidly began to diminish; in ten days it disappeared completely, and in ten days afterwards the different dropsical effusions disappeared also. In one of the cases, the treatment having been suspended a little too soon, the albumen again appeared in the urine. In order to judge comparatively of the effects of the perchloride and the ergot, the perchloride was administered alone, when the albumen diminished; but this diminution, although rapid at first, was afterwards very slow. The ergot being added to the prescription accelerated the cure, and four days after its administration there was no more albumen in the urine. M. Perrond, in making some remarks on these cases, observes that the ergot and the perchloride of iron appear to have a beneficial effect on the albuminuria, but that their use constituted the treatment of a symptom rather than that of a disease, and that they are not therefore calculated to supersede the use of other measures intended to remove the original malady.

ART. 82.—*Spontaneous Coagulation of Urine.*

By Mr. A. W. STOCKS, Surgeon to the Salford Hospital.

(*Medical Times and Gazette*, January 21, 1865.)

CASE.—J. W., aged 27, warehouseman, married; sanguine temperament; has a fine, soft, transparent skin; has had gonorrhoea and iritis; never had syphilis; otherwise well. About nine years ago, had a dry, scaly eruption over the whole body, desquamating in large patches. The nails became corrugated and thickened by a deposit, like plaster of Paris, under them. Had sulphur baths twice a week, and recovered in about three months.

July 6th, 1862.—Has a severely-itching eruption, like urticaria, over the whole body, most intensely on the back of the hands and perineum, extending from the latter situation up the groins and down the inside of the thighs. The skin on the palms of the hands is hard and dry, cracking at each flexure, and there exudes from each crack a thin watery fluid. He had warm and Turkish baths, which greatly aggravated his distress.

August 12th.—As the symptoms now seemed to divide themselves into two classes, viz., those connected with the skin and those connected with the urinary organs, I have arranged them accordingly.

The eruption on the skin is now vesicular, discharging a thin, straw-coloured fluid, which, drying up, covers the whole body with scales, most severely on the extremities, comparatively little on the trunk. When these

are removed, the skin is seen, red, thin, tender, and covered with small papules, and a thin fluid exudes rapidly, which, drying again, forms scales. The palms of the hands and soles of the feet are covered with the thick cuticle, with a pulpy feeling underneath, and when pressed upon, there exudes a large quantity of milky fluid from the cracks at each flexure. The skin of the sole of the feet came off *en masse*. This process was repeated during the attack. All the nails came off, and were replaced by brittle masses, thickened, as if by a layer of plaster of Paris, underneath, and corrugated, like an oyster-shell; they gradually became healthy. All the hair fell off. The mucous membrane of the mouth is red and tender. The conjunctivæ are reddened, and the eyelids constantly adhere. From each ear exudes a thick, yellowish discharge, which, gradually becoming thinner, at last ceased. There was numbness and pain in the fingers and toes, which were useless for three months; and though the fingers are much better, the numbness and pain in the toes continue even now, but more moderately. There was intense itching at first, but that gradually disappeared.

This state of matters continued from August until the middle of December, being most intense in October; and during this period the following symptoms were observed connected with the bladder, &c. :—

There is great pain over the kidneys, in the perineum, and about the anus, the passage of fæces aggravating the latter much. There is no tenderness in the prostate. There is continuous oozing of urine, with frequent and painful micturition, about half an ounce of urine passing at each attempt. Masses, about the thickness of an ordinary lead pencil, one inch to one and a-half inch long, very like pieces of tripe, were pulled from the urethra three or four times a day for a week. The urine itself is clear, highly albuminous, and, when allowed to stand in a vessel, coagulated spontaneously into yellow transparent masses, exactly like half-melted calf's-foot jelly, floating in the fluid part of the urine—about half the urine being solid and half liquid. These masses again became fluid in about twenty-four hours, leaving floating in the urine thin cobwebby films. This continued about two weeks, the spontaneous coagulation ceasing, but the albuminuria continuing.

Under the microscope ($\frac{1}{4}$ -inch), the white masses appeared to consist of wavy lines interlacing each other in all directions, and in great abundance, entangling numerous epithelial scales, and pus, and blood corpuscles.

The yellow jelly-like masses had the same appearance, except that the wavy lines were far apart. In the former case, the lines covered the whole of the field thickly; in the latter, only three or four lines appeared, the interstices being filled apparently with clear urine.

The cobwebby films appeared to be the fibres of the last fallen together, the fluid part having exuded. There were no fibrinous casts of the uriferous tubes to be found.

These symptoms continued for a similar period to those of the skin, but the above peculiar appearances were seen only during October, when the skin disease was at its height; and at the present time the urine is slightly albuminous, containing a few pus corpuscles. The micturition is very frequent, and there is occasional severe heavy pain in the perineum, increased by exposure to cold.

The treatment consisted of infusions of *pareira brava*, *uva ursi*, *triticum repens*, opiates, diaphoretics, and belladonna suppositories, but the greatest relief followed the exhibition of *tr. ferri sesquichloridi* in drachm doses thrice a day, with generous diet; and the irritation of the skin was much allayed by

the use of a lotion composed of a drachm of sesquicarbonate of soda dissolved in a quart of water.

Although the symptoms seemed to divide themselves into two classes, I am inclined to think that they arose from one disease—namely, an inflammatory eczema attacking both skin and mucous membranes; and that as fibrinous exudations were poured from the skin, in like manner fibrin was poured out of the mucous membrane of the bladder sufficient to produce the appearances related above.

ART. 83.—*On the Best Method of Detecting Small Quantities of Albumen in the Urine.*

By DR. ANDREW CLARK, Assistant-Physician to the London Hospital.

(*Clinical Lectures and Reports of London Hospitals*, 8vo., 1864.)

It is of great clinical importance to possess a simple and certain method of detecting small quantities of albumen in the urine. After a variety of experiments, Dr. Clark has come to the conclusion that nitric acid used in the manner about to be described—a modification of the plan suggested by Heller—is by far the most sensible, reliable, and handy agent that can be used for this purpose by the physician.

Pour not less than half a drachm of *fuming* nitric acid into a test tube; incline it, and then let a like quantity of the suspected urine trickle down *very slowly* to the acid, over the surface of which the urine will float without the slightest admixture. If albumen be present, a milk-white, sharply defined, tolerably tenacious film will form at the exact point of junction of the two fluids. This film is never, at first, thick; and when the amount of albumen in the urine is extremely minute, it may be so thin as to become visible only by reflected light when the test tube is inclined. Occasionally, when very thin, the albuminous film is dissolved in the course of a few hours. Commonly, however, it increases in breadth, diminishes in density, becomes yellow or yellowish-green at its under surface: and throws off minute coagula, which fall through the acid to the bottom of the tube.

Nitric acid used in this manner as a test for albumen is also a test of the presence of uroxanthine, or bile—either or both of which are not unfrequently present in temporary and functional albuminuria.

If, in immediate contact with the acid, a ruby or violet ring is developed, uroxanthine is present; and bile also, if in addition to a red or violet there is formed likewise a green-coloured ring, which remains for some time.

Two feasible objections are urged against depending solely on the employment of nitric acid in the manner described, as a test of the presence of albumen; and Dr. Clark himself has noticed a third; but a careful examination of their force leads me to the conclusion that they are more theoretical than real.

When urine, rich in uric acid or its salts, or containing much scaly epithelium, is poured over cold nitric acid, a general turbidity arises, which is said to be undistinguishable from that produced by the presence of albumen.

But if the proposed test for albumen be properly applied, no turbidity will be produced by the presence of that substance, unless urates are also present. And then the white film of albumen is separated from the superimposed turbidity by a thin stratum of clear urine.

The turbidity produced by uric acid or epithelium is general, granular-like, and without any approach to coherence.

The turbidity produced by urates is sometimes abruptly defined below by an opaque, ring-like border, sometimes coloured, sometimes not; but a stratum of clear urine intervenes between this ring and the surface of the acid, and, as above, the turbidity has no cohesion of parts. Besides this, the turbidity produced by lithates may be immediately dissipated by heat; and, if not in great excess, even by the heat of the hand closed around the tube.

The film produced by the contact of nitric acid with albuminous urine is quite different from any kind of turbidity. Confined to the layer of urine resting upon the acid, white like a disc of compressed cotton, tenacious, and, when shaken with its associated fluids, breaking into flaky fragments, it seems improbable that any but the merest tyro should mistake it for anything but what it is.

In testing for albumen by means of heat and nitric acid, there may be no immediate response indicative of its presence; and yet after a few hours, a flocculent precipitate may form and fall to the bottom of the tube.

A specimen of urine examined within an hour after extrusion from the bladder, may yield unequivocal evidence of the presence of albumen, and cease to do so after twelve hours.

Small films of coagulated albumen produced on the surface of nitric acid, occasionally disappear within a few hours from the time of their formation.

Little importance is to be attached to the presence of small quantities of albumen in the urine of women a day or two before or after menstruation. It is common without any disorder of the kidney, or any sensible discharge from the vulva.

Small quantities of albumen are often present in the urines of women with leucorrhœa, and of those who have recently had fits of hysteria.

One is not justified in asserting the absence of albumen in the urine upon the result of one or two examinations. Dr. Clark knew a case in which albumen occurred in the urine daily for several months; but it was present only in the urine first passed after breakfast, and was never, to the time of its departure, present in the urine passed at any other time.

Men sometimes discharge a thin whitish glairy fluid with the closing stream of urine in the act of emptying the bladder. This fluid is said to be seminal; but in none of the examples examined were any spermatic filaments present. From its containing mucin,

and young cell particles, Dr. Clark looks upon it as an augmented and slightly altered secretion of the glands opening into the urethra. When discharged in any quantity, the urine containing it responds to all the ordinary tests of the presence of albumen.

Albumen in small quantities and unaccompanied by casts, may be present in the urine daily for three years, and at last permanently disappear. This occurred in a case under Dr. Clark's observation. The health, which had previously been bad, rapidly improved after the disappearance of albumen from the urine, and became ultimately very good.

Mere hepatic congestion is sometimes the cause of slight functional albuminuria. Dr. Clark had under observation for some time a lady whose "liver attacks" were invariably preceded by the appearance of small quantities of albumen in the urine. With the free purgation which was found necessary for the removal of these attacks, the albumen disappeared. We remember also the case of a gentleman who was subject to somewhat similar attacks. In his urine, however, free uric acid was associated with the albumen, and both stayed several days beyond the subsidence of acute disorder. But he was a wilful patient, and chose to live well even at the cost of being ill.

ART. 84.—*On the Urine of the Insane.*

By Mr. ADAM ADDISON, Resident Medical Officer,
Montrose Royal Asylum.

(*Medico-Chirurgical Review*, April, 1865.)

Mr. Addison gives, so far as the urine is concerned, the particulars of forty-nine cases of various forms of mental disorder, and his deductions from the inquiry as a whole are these:—

1. That the quantities of the urine, of the chloride of sodium, urea, phosphoric and sulphuric acids, excreted during the course of a maniacal paroxysm, occurring in acute mania, epilepsy, general paralysis, melancholia, or dementia, are less than the amounts excreted in an equal time during health.

2. That in chronic melancholia the quantities of the chloride of sodium, urea, phosphoric and sulphuric acids are reduced below the mean, and sometimes the minimum of health.

3. That in idiocy, dementia (paralytic and common), the urea, chloride of sodium, and sulphuric acid range above and below the normal mean of health; that in some cases the amount of phosphoric acid is greater than the mean according to weight, but in the majority of cases it ranges between the minimum and mean found in healthy adult men.

(F) CONCERNING THE CUTANEOUS SYSTEM.

ART. 85.—*On the Geography and Prophylaxis of Tinea.*

By Dr. L. J. BERGERON, Physician to the Hôpital
St. Eugénie, Paris.

(*Annales d'Hygiène Publique et de Médecine Légale*, January, 1865.)

Dr. Bergeron, after a careful study of the distribution and etiology of *Tinea*, has arrived at the following conclusions:—

1. From an examination of the conditions which give rise to the propagation of many maladies, it may be concluded that public hygiene will, sooner or later, achieve the destruction of the morbid principle of some of them, and root out the foci of infection of others: but the pathogeny of parasitic maladies, and in particular the pathogeny of *tinea*, is now sufficiently established that the measures proper for the eradication of this class of diseases may be indicated and organized.

2. In order that these measures should be rapidly and completely efficacious, three things are organized; namely, (1) a search for *tinea* wherever their existence is suspected; (2) the isolation to a greater or less extent of the individuals attacked; and (3) prompt treatment of the sick.

(a.) So far as concerns the search for the habitats of *tinea*, in France, medical geography has as yet given only an imperfect knowledge. The statistics in existence on the subject refer only to departments. But while waiting until a special and more minute inquiry can be obtained, so that the distribution of the maladies in the different cantons could be ascertained, Dr. Bergeron asks why *tinea* should not be sought for and eradicated in public establishments, asylums, schools, and workshops?

(b.) The admission of individuals suffering from any variety of *tinea* into public institutions devoted to children is formally forbidden; but the interdiction is not rigorously carried out. Provision for the more rigid execution of the law, and for a systematic inspection of the institutions, would cause a rapid diminution of the spread of *tinea*.

(c.) As to prompt treatment, without which the foregoing measures would be incomplete, this would only be possible when the administrative commissions of many provincial hospitals shall have modified or abrogated a rule which excludes patients suffering from *tinea* from the benefits of treatment in the institution; and when the departments shall have set apart sufficient means for the organization of a service for the treatment of *tinea* in the chief towns of the Arrondissements, on a basis as liberal as that adopted at Paris, Lyons, Bordeaux, and other large towns; and also for the institution of gratuitous external treatment in the chief towns of cantons, and in the largest communes.

It is probable that a premium accorded after a complete cure would have the double advantage of inducing poor families to sub-

mit their children to treatment, and induce them to persevere with the treatment to the end.

3. Of the two species of true or parasitic tinea which are most frequently observed, the *tinea favosa* (*Achorion Schönlinii*) is most frequent in the country; the *tinea tonsurans* (*Trichophyton tonsurans*) in towns. This double fact, which appears to be contrary to the data of comparative pathology, shows the need of further investigations concerning the origin of forms.

4. An analysis of the statistical documents furnished by the *Compte rendu sur le recrutement l'armée*, demonstrates that *tinea favosa* tends to diminish in frequency in the majority of the departments, but that this declension is, as a rule, very slow.

5. It is shown by the same documents, that some of the departments are completely free from the tinea, and that this affection is distributed very unequally. In the South, for example, and around *Herault*, in which the greatest number of rejections for this cause occur (twenty in every thousand subjects examined by the revising councils), radiate a certain number of departments, in which the proportion of rejections approaches the preceding. In the North-west several departments are grouped around the *Seine-Inférieure* and the *Somme*, in which the rejections for tinea are as numerous as in the South. But in the centre of France, on the contrary, tinea is very rare, and it is rarer still in all the region of the North-west. In the department of the *Haut Rhin* the number of exemptions is less than a unit (0.85) in every thousand examined.

6. The causes, probably complex, of this distribution of tinea, are difficult to be determined in a precise manner; but it may be presumed that everywhere neglect, ignorance, and misery, powerless in giving birth to the disease, concur at least in maintaining and propagating it. It does not follow, however, that the frequency of tinea is at all times an exact measure of the wealth and instruction of the population of a department. Some data lead to the inference that the general average of tinea in the larger districts, shown by the official documents, is furnished only by certain more instructed localities in which the population has lagged in its progress.

ART. 86.—*On the External Use of Creasote in the Treatment of Sycosis.*

By Dr. MASSE.

(*Gaz. des Hôp.*, and *Glasgow Medical Journal*, January, 1865.)

CASE.—The patient, at the time an inmate in the Hôtel Dieu at Montpellier, was a young soldier who had a pustulo-tubercular eruption upon the cheeks and chin; he exhibited, moreover, *herpes circinatus* upon the back of his hand. He had contracted this malady while being shaved by a barber whose razor or linen, plentifully supplied with the parasites, had thus communicated the same affection to five or six other soldiers. I coupled microscopical examination with the information I had acquired as to the etiology of this affection, with a view of completely investigating the nature of it. I

discovered, in the hypertrophied hair-bulbs, spores and tubes of the mycelium of a veritable *Microsporon mentagrophyte*. The treatment employed consisted of lotions, composed of 50 grammes of water, 50 grammes of alcohol, and 50 centigrammes of creasote, and which were used twice a day for a week. At the end of this period the amount of creasote in the lotion was doubled, the latter being applied three times a day. By the end of another week the pustulo-tubercular eruption had completely vanished. Microscopical examination failed to exhibit any parasites, and the invalid had completely recovered. This novel method which I came to make trial of cannot be said to be empirical treatment, since it is but a medical adaptation of the recent investigations of M. Béchamp upon the action of creasote in fermentation, and according to which creasote was found under these circumstances to check the development of the spores of the vegetable cryptogams, as well as of the ovules of the infusoria. The cryptogamous parasites appearing to me, viewed with regard to their organization, to differ but little from the mould developed during fermentation, I thought that creasote might in a like manner arrest the formation of parasitic spores in diseases of the skin. By destroying the source of a malady we should effect a cure. Creasote does not immediately destroy the cryptogam of which the mycelium is already developed, but it renders the spores incapable of germinating; after a short time the mycelium becomes dried up, and the parasite disappears.

ART. 87.—*On Eczema of the Eyelids, Conjunctiva, and Cornea.*

By MR. FURNEAUX JORDAN, Surgeon to the Queen's Hospital, Birmingham.

(*Proceedings of the Roy. Med.-Chir. Soc.*, January 10, 1865.)

Many observers, and especially writers on diseases of the skin, have considered ophthalmia tarsi to be simply eczema of the lids. Dr. Mackenzie has pointed out that scrofulous, or, as he terms it, phlyctenular ophthalmia, is frequently associated with eruptions on the skin. It is the object of this paper to show that not only ophthalmia tarsi is eczema of the lids, but that granular lids, a peculiar swelling of the sub-integumental connective tissue of the lids, lippitudo, strumous ophthalmia, certain forms of simple or catarrhal ophthalmia, keratitis and strumous keratitis, and certain ulcers on the cornea, are merely varieties of eczematous disease. Cases of extreme, firm, indolent, pale or pinkish swelling of the lids, occur occasionally, the only cause of which is eczema of the margins of the lids. The eczema may be very slight, or it may pass away quickly, and leave only the swelling behind. Unchecked eczema of the eyelids terminates in lippitudo, just as persistent and progressive eczema of the cornea produces pannus. Both these conditions are analogous to the eczematously red, swollen, and moist condition of the skin which may persist for an indefinite period. Eczema of the conjunctiva presents many important features. The so-called strumous ophthalmia may be regarded as chronic eczema. The several stages of pimple, vesicle, ulcer, or thickened patch, admit of indisputable demonstration. In acute eczema of the conjunctiva, there

is for a few days a uniform scarlet colour; then a crowd of vesicles, which soon pass away, and leave an irregular or patchy redness—each patch, however ill-defined, having a redder, thicker, and possibly ulcerated centre. These cases have a slight muco-purulent discharge, and are always tedious. If treated as eczema, they speedily recover. The so-called keratitis, or strumous keratitis, is eczema of the cornea. When vesicles, white patches (necessarily white because of the anatomical structure of the cornea), or ulcers occur on the cornea in conjunction with vesicles on the conjunctiva, the term *scrofulous ophthalmia* is commonly used. If the same pimples (necessarily flat), vesicles, patches, or ulcers occur on the cornea alone, especially near its centre, the term *keratitis* is applied, notwithstanding the symptoms are similar, and notwithstanding that there is usually, if it be carefully sought for, evidence of eczema of the lids or face, or ears or scalp. The characters of eczema of the cornea are quite as typical as they are of eczema elsewhere. The several varieties of eczema of the cornea, conjunctiva, and lids are combined in a great variety of modes. They are much more frequently combined than not, and very frequently indeed associated with cutaneous eczema in its favourite localities. Eczema is often limited to sites as small as the cornea. The treatment should be directed to eczema. Its chief features are non-stimulating diet and alkaline medicines, with a little iron added in most cases. If the lids are affected, as also in pannus, lippitudo, and granular lids, a little of any of the "eczema ointments" may be used, with the customary attention to details; if much photophobia, a little morphia may be given in the morning.

ART. 88.—*On Herpes Zoster.*

By Dr. SCHMIDT.

(*Berlin. Klin. Wochenschr.*, No. 40, 1864.)

Dr. Schmidt relates a case of this disease which is interesting in reference to the doctrine of V. Bärensprung, that herpes zoster depends upon an affection of the ganglia on the posterior roots of the spinal nerves.

CASE.—The patient, a porter, forty years old, fell down a staircase ten feet high, shortly before Christmas, 1863. He was severely bruised on the loins and left thigh, and was stunned by the fall, but soon recovered himself and crawled home. After eight days of rest in bed, he was able, with difficulty, to resume his employment, but still suffered from a fixed circumscribed pain referred to the region over the middle lumbar vertebra, and extending some inches to the left.

Three weeks after the fall, the patient was suddenly attacked by febrile symptoms, and by an increase of the pain, with extension of it towards the inguinal region. On the following day, an eruption of herpes appeared on the left lumbar region, extending to the upper part of the thigh, scrotum, and groin. Between the clusters, the skin was very insensitive to a needle point, as compared with that of the opposite side. The patient speedily

recovered, and the lumbar pain disappeared with the disappearance of the herpes.

The appearance of the eruption coincided precisely with the parts supplied by the cutaneous branches of the ilio-hypogastric, ilio-inguinal and genito-crural nerves, and of the superior posterior gluteal nerves; and might very well have been dependent upon a morbid condition of the ganglia of the first and second lumbar nerves.

ART. 89.—*On Lupus Erythematodes.*

By Dr. ISIDOR NEUMANN.

(*Wien. Med. Wochens.*, xiii., 41, 1863; and *Schmidt's Jahrb.*, 124, 1864.)

Dr. Neumann has made careful microscopical examination of portions of diseased skin removed, from time to time, from a patient suffering from lupus erythematodes of the head and face. He concludes that the disease consists essentially of a chronic inflammation, during which a new formation of nuclei takes origin from the corpuscles of the connecting tissue. The clusters of nuclei gradually invade all the textures of the skin, destroy the vessels, produce wasting of the nerves, and impede or arrest the actions of the sebaceous or sudoriparous glands. The excretory ducts of these glands, especially of the former of them, are obliterated by the pressure they sustain from increasing nuclei proceeding from the rete Malpighii, and become closed cavities, with their functions suspended. The hairs fall out, the adipose tissue disappears, and in time the disease produces sclerosis of the skin, with the characteristic parchment-like appearance.

ART. 90.—*On "Soor" and its Peculiar Fungus.*

By Dr. BURCHARDT.

(*Annalen d. Berl. Charité*, xii., 1864; and *Schmidt's Jahrb.*, 125, 1865.)

Under the name of "soor," Dr. Burchardt describes a disease of the mucous membrane of the mouth, caused by the presence of a fungus, and chiefly affecting children during the first year of life. The fungus forms a milk-white deposit upon the mucous membrane, destroys the epithelium, produces slight inflammation in the membrane itself, and, when abundant, may interfere with sucking and swallowing, or with voice and respiration if it should invade the vocal cords. The author gives a full account of the microscopic characters of the fungus, and concludes his memoir by saying that the most rapidly curative treatment is to wipe away the growth six or eight times a day, by means of a piece of soft linen rag. The application of borax has produced no beneficial results in his hands.

PART II.—SURGERY.

SECT. I.—GENERAL QUESTIONS IN SURGERY.

ART. 91.—*On the Efficient Treatment of Hospital Gangrene.*

By Dr. PACKARD, of Philadelphia.

(*American Journal of Medical Science*, Jan., 1865.)

There are two indications always present in cases of this kind. First, all the putrid and putrescent matter must be removed from the wound. Secondly, means must be taken to prevent the recurrence of the sphacelation or necrosis, into which the tissues will surely run if not checked.

To fulfil the first indication, the forceps are sometimes all that is needed. Dr. Packard's practice is, to seize a portion of the gangrenous connective tissue, and then twist and roll it up, with traction, until it comes away as far as possible, without too great force. Sometimes it will either break or come away; but if it still resists, it must be cut away with a pair of scissors (a bent or curved pair will usually answer best), or with a knife. This must be repeated again and again, until all the tenacious putrilage has been removed. Rough sponging will still further cleanse the surface.

Chloroform or ether should be used in all cases where there is much pain or tenderness in the sore, or when the wound passes deeply through a thick fleshy part, or is undermined at its edges. *The whole surface of the wound should be reached and cleansed; and the surgeon has not done his work if he stops short of this.*

It will do no harm, after this cleansing, to "disinfect" the wound by washing it with the solution of chlorinated soda, with bromine, with a solution of permanganate of potash, or with any other preparation of the kind. But that is not necessary. The thorough cleansing by mechanical means is the main thing to be attended to.

As to the second indication, it is to be met simply by using as a dressing, a preventive of oxidation. Sugar, a hydrate of carbon, which does not give up its oxygen, and which is well known for its preservative powers in the case of meats and fruit, is admirable for this purpose. Powdered white sugar is thoroughly and thickly dusted over the wound, or a thick syrup is put on like any other wet dressing, by saturating clean rags with it. Dr. Packard prefers the former method, the sugared surface being covered with wet lint

or rags, kept in place by adhesive plaster, or by strips of bandage tied just tightly enough to keep their place.

Coal-oil, turpentine, or any other carbo-hydrogen, if pure, would answer, but the sugar is less offensive, and does not give pain. A mixture of pulverized charcoal with the sugar answers very well when the odour does not quickly disappear after the cleansing.

Wounds still healthy may be prevented from becoming foul and gangrenous from the neighbourhood of those which are in the latter state, by the use of sugar or any pure carbo-hydrogen as a dressing, and that the spread of hospital gangrene in a ward may be thus checked.

The cleansing may have to be repeated once, perhaps in some cases twice, before the wound assumes a healthy aspect, but whenever the whole surface can be reached at the first time, this will probably be sufficient. To prevent the spread of the disease by contagion, it is absolutely necessary that each case should have a special sponge and basin set apart for itself, and that these articles should be regularly and thoroughly cleansed after each dressing. Boiling water will effect this.

In conclusion, Dr. Packard says :—

"I beg to be excused for again urging the conviction, that, although hospital gangrene may disappear under the use of the actual cautery, of nitric acid, or of bromine, the happy issue is in such case in spite of the remedy, and not in consequence of it; that the *cure* consists in the removal of all the sloughing and dead tissues, and the *prevention* in opposing oxidation by means of a dressing with any substance which either contains no oxygen or which will not give it up. Once more I must express the entire confidence which a very extended observation has led me to bestow upon this theory, and the resulting line of practice."

ART. 92.—*On the Prevention of Suppuration after Operations on Tumours.*

By M. PÉTREQUIN.

(*Gaz. Méd. de Paris*, Dec. 10, 1864.)

At the meeting of the Academy of Sciences on November 28, M. Velpeau communicated a note from M. Pétrequin, in which that surgeon advocated the application of tincture of iodine as a means of preventing suppuration after the removal of tumours; especially in situations such as the face and neck, where it is desirable to prevent the formation of cicatrices. Hitherto, M. Pétrequin observed, iodine has been applied with the view of modifying the suppurative process; but M. Pétrequin's object has been to prevent it altogether. He has, like M. Velpeau, many times observed that, in hydrocele, for instance, suppuration was less likely to follow the injection of tincture of iodine, than of wine. He has never seen suppuration follow the injection of iodine into the parenchyma of organs, into

glands, into the thyroid body, or into cavities; but, on the contrary, the formation of pus appears always to have been prevented.

ART. 93.—*On the Combination of Distal with Proximal Compression in certain Cases of Aneurism.*

By Mr. J. M. O'FERRALL, Chief Surgeon to St. Vincent's Hospital.

(*Medical Press*, March 15, 1865.)

In a clinical lecture on this subject, Mr. O'Ferrall says:—

"I have long since been of opinion that compression of an artery on the distal side of an aneurismal sac should precede or accompany that on the cardiac side of the aneurism. I have often observed arrest of pulsation easily accomplished, attended at the same time with a flaccid state of the sac; but I have also remarked in such cases that the moment the pressure ceased the sac filled and throbbed as before. The formation of the coagulum is thus desirable as the first step to consolidation. A half empty sac is the ready recipient for the slightest thready current that can follow the cessation of the compressing force on the cardiac side. It, therefore, appeared to me advisable to interrupt the current only when the sac was full of blood. These considerations, together with the knowledge of the fact that ligature of the artery on the distal side has sometimes cured an aneurism when the upper or cardiac portion of the vessel could not be reached, made me resolve to try this expedient whenever a fair amount of pressure was not followed by success. From what I have observed, I am inclined to think that cessation of pulse in a sac which suddenly diminishes in size and becomes flaccid, is less likely to be followed by a permanent cure, whatever time may have been occupied in the compression; and that a sac which retains its volume, and is, moreover, full of coagulum, is less likely to be refilled, however short the duration of the compressing force.

"When the supply is completely cut off by ligature of the trunk, I believe that the danger of relapse from refilling of the sac by collateral sources is more likely to occur when the sac collapses and becomes flaccid at the moment of deligation than when its dimensions are unchanged. This firmness of the parietes of the sac is always considered favourable to the success of the operation, as implying the presence of fibrinous deposits, whether we attribute them to stasis of the blood or to inflammatory exudation, as suggested by the researches of that distinguished surgeon, the late Dr. Abraham Colles. I have no doubt, however, that whether a sac be filled by fibrinous deposits of some duration, or by coagulum recently formed, that a full sac is very influential in preventing the ingress of blood from a compressed artery above, or collateral branches from below; the compression below the sac need not, in some cases, last more than a few minutes before the current above is stopped. If, on making the pressure above and arresting the pulsation the sac

remains full, the object is attained; and time is merely required to allow the blood, then liquid, to coagulate in the sac. In other cases, it may be prudent to continue both compressions for a longer time. Three cases have already been treated on this plan with success. The first was published by me in the *Dublin Quarterly Journal* for November, 1846."

ART. 94.—*Sudden Death resulting from Injection of
Perchloride of Iron in cases of Nævus.*

By Mr. R. BRUDENELL CARTER, of Stroud.

(*Journal of Practical Medicine and Surgery*, Feb., 1865.)

Mr. Carter relates the two following cases:—

CASE 1.—The first patient, a little girl, aged two months, bore at the lower part of the nose an agglomeration of nævi, which involved both alæ and the septum. Cauterization with a heated needle and the application of elastic collodion were in vain resorted to. Vaccination had already been performed; pressure or ligature were inapplicable, and escharotics might occasion a fatal hæmorrhage. Excision would entail the total removal of the nose, and the limits of the disease were not known. The surgeon therefore determined on having recourse to the injection of sesquichloride of iron. The operation was twice performed, and on the first occasion without any evil consequences; but on the second, the piston of the syringe did not work well, and five drops of the solution were suddenly thrown into the tumour. A livid patch immediately appeared above the point into which the syringe had been inserted, the infant uttered one scream, and expired after a short convulsive attack.

CASE 2.—The second case occurred recently in a colonial hospital, and the post-mortem examination showed that the point of the canula (the nævus occupied the cheek) had penetrated into the transverse facial vein, and that coagulation of the blood in the right cavities of the heart had been the immediate consequence. Mr. Crisp, of Swallowfield, who forwards the notes of the case, does not state what amount of liquid was injected, nor with what degree of strength the injection was thrown in.

Hence, Mr. Carter concludes, in opposition to the views entertained by Mr. Erichsen and M. Testelin, that in subcutaneous tumours the surgeon should not resort to injections of sesquichloride of iron unless it be possible, by pressure, acupressure, or temporary ligatures, to cut off for a short time all communication with the afferent veins. "With these precautions," says Mr. Carter, "the remedy is harmless, and will generally be found efficacious; but if they cannot be resorted to, the injection is so dangerous, that for my part I could not, except under very exceptional circumstances, make up my mind to perform it."

ART. 95.—*On the Question of Connecting the Ends of a Divided Nerve by Suture.*

By Drs. EULENBERG and LANDOIS.

(*Berlin. Klin. Wochenschr.*, I., 46, 47, 1864.)

In consequence of the case by M. Nélaton recorded in the present volume, (Art. 132) and of the previous case by M. Laugier (xl. p. 200), Drs. Eulenberg and Landois have performed several experiments upon various animals, and in every instance they have not only failed in obtaining union of the divided nerves, but the section has often been followed by their degeneration, or by neuritis or perineuritis with suppuration, which has even led to pulmonary metastatic abscess. They conclude that the general applicability of the method of Laugier and Nélaton is problematical; and that further experience of it is required before any decision upon its merits can be formed.

ART. 96.—*On Osteoplasty.*

By Dr. KADE.

(*Petersb. Med. Ztschr.*, iii., 1 & 2, 1852; and *Schmidt's Jahrb.*, 124, 1864.)

The word osteoplasty was first used by Pirogoff to denote his modification of Syme's amputation at the ankle-joint; and it was afterwards used for a class of operations similar in principle, in which cut surfaces of bone were opposed and made to unite, such as resection of the elbow and of the knee. Langenbeck, however, employed the word in a different sense, to denote his first case of rhinoplasty with transplantation of the frontal pericranium, and afterwards for two cases of "osteoplastic" resection of the upper jaw, and for his first case of uranoplasty. Hence the meaning of the word has become obscure. It is evident that there is nothing truly osteoplastic about Pirogoff's amputation, or about resection of the knee, or in separating portions of the upper jaw, and replacing them after the removal of a tumour. For the future, the term osteoplasty should be applied only to operations in which periosteum is transplanted for the purpose of producing new bone, as in the nose and palate cases mentioned above.

After tracing down the history of our knowledge of the power of periosteum to produce bone, and after referring to the experiments of Flourens, Dr. Kade describes the more recent ones of Ollier. He detached strips of periosteum from the tibiæ of rabbits, leaving them adherent to the bone at one end only, and wound them about among the muscles and under the skin in various directions: thus obtaining new bone of any desired shape. In a second series of experiments he completely severed the connexion between the strip and the bone on the fourth day; and, in a third, he severed this connexion in the first instance, and transplanted

the periosteum to various parts of the body. In all cases a formation of bone was the result; not a mere calcification of connecting tissue, but a formation] possessing all the characteristics of osseous structure.

The first application of this property of the periosteum to operative surgery is due to Langenbeck; although the suggestion so to use it was made by Ollier. Malgaigne in 1834 advocated the preservation of the periosteum in resections, on the ground that in children it formed a basis for new bone, and in adults for fibrous tissue; and Sexter, sen., in 1839, preserved the periosteum of a partially carious rib with full knowledge of the results to be obtained from doing so. Since then the same thing has been done by many surgeons; but the transplantation of periosteum is manifestly a step in advance of its mere preservation.

The results hitherto obtained from osteoplastic operations have been very satisfactory. Langenbeck has recorded two cases of rhinoplasty; in the first of which firm new bone was formed in four weeks, and in the second in eight. Dr. Kade saw him operate on a third case, and saw the patient again six weeks afterwards. The operation had been very difficult and tedious; and the point of the new nose had sloughed. At that time no bone could be discovered in the bridge.

On the hard palate the results have been still more favourable; and Dr. Kade refers to eleven cases of uranoplasty by Langenbeck, and to one case of his own, performed in January, 1862, in which he fully describes the several stages of the operation. The patient was a peasant lad seventeen years old, with congenital fissure of the lip, hard palate, and velum, on the left side. The staphyloplasty and uranoplasty were both accomplished at one sitting.

The first step of the operation was to pare the edges of the fissure. In doing this the knife was carried from behind forwards and from below upwards, so that parts still uncut were not obscured by bleeding. In consequence of the scanty development of the velum it was impracticable to remove its margin in a continuous strip; and the paring was done piecemeal, and with some difficulty. The fissure of the palate itself was pared more easily, and the operator found it most convenient to stand behind the patient, supporting his head upon his breast. The same position served for the lateral incisions and for the separation of the periosteum. After paring the fissure the author proceeded to make his lateral incisions; thus departing from the practice of Langenbeck, whose second step is the section of the muscles of the velum. Dr. Kade defends his own practice partly because he thus proceeds continuously with what can be done from behind the patient, and chiefly because the myotomy is attended with acute pain and free bleeding, and requires a long interruption of the operation. In the case under consideration, the cleft of the hard palate being unilateral and not very wide, and the palate process of the superior maxilla not too small, it was sufficient to make a single lateral incision, on the side on which the palatine process was not united to the vomer. The third step, the separation of the muco-periosteal covering of the palate, should be

commenced on the right side from the margin of the cleft, on the left side from the side of the teeth. When the muco-periosteal covering has been separated by an elevator as far back as the posterior margin of the palate plate of the palate bone, the mucous membrane of the velum must next be separated from this margin. For this purpose a straight probe-pointed knife should be carried along the margin from within outwards, as far as the hamular process. In raising the periosteum, it is necessary to be very careful to avoid the incisive foramen and the posterior palatine foramen, which give passage to the nutrient vessels. It is easy to avoid the naso-palatine artery, which passes through the incisive canal, since the fissure is usually small anteriorly; and no very extensive separation of periosteum is there required. In experiments on the dead subject, the author found the anterior palatine artery, which passes through the posterior palatine foramen, always uninjured, even although he separated the periosteum with but little care. He thinks, moreover, from the very free anastomosis upon the palate, that the vitality of the flaps would not be endangered, so long as one of the nutrient vessels remained intact. The fourth step is the introduction of the sutures, which can be done more easily while the velum is tense than when it has been relaxed by myotomy. The author used nine sutures, five in the palate and four in the velum. As each thread was passed, he stuck together its free ends by a bit of wax and gave them in charge to an assistant. The fifth step was the myotomy, for which a sickle-shaped knife was thrust through the velum, below and somewhat external to the hamular process, towards the posterior wall of the pharynx, and the velum was then divided in its whole thickness, by sawing movements of the knife, up to the posterior margin of the palate bone. It is desirable to divide the levator and circumflexus palati at a distance from the median line, where they are small, and prior to their fan-like expansion; but it is an error to suppose, with Langenbeck, that the palato-pharyngeus can be divided by the same incision. The author thinks that Langenbeck's incision is insufficient to relax the velum; and that the palato-pharyngeus or posterior pillar, and the palato-glossus or anterior pillar, require to be separately divided. He neglected to do this, and the fissure of the velum did not unite after his first operation, but only after a second, in which the pillars were cut through. After the myotomy the patient requires a period of rest; and then the operation is completed by tying the sutures.

In the author's case the fissure of the palate was perfectly closed, and the second (successful) staphyloraphy was performed a month after the first. In March the hare-lip was united, and healed by the first intention; and, at the end of the same month, a needle discovered new bone in the place of the former cleft of the hard palate. The speech was still defective, the patient being stupid, and having neglected to carry out the necessary vocal gymnastics.

ART. 97.—*Remarks upon Osteo-Myelitis consequent on Gunshot Wounds of the Upper and Lower Extremities, and especially upon the Treatment of Stumps affected with Osteo-Myelitis after Amputation necessitated by such Injuries.*

By Mr. THOMAS LONGMORE.

(*Proceedings of the Royal Medical and Chirurgical Society, Feb. 20, 1865.*)

The author commences his communication by noticing the particular interest which had been excited amongst military surgeons, especially French surgeons, during the last few years in the subject of osteo-myelitis, or endosteitis, as it is called by some writers, after gunshot wounds of the extremities, and of its proper treatment. The interest arose, not from any belief that a difference existed between the nature of the inflammation of the medullary tissue when developed after gunshot injuries, and the corresponding inflammation occasionally seen after the ordinary injuries and amputations of civil life, but from the comparative frequency of its occurrence after gunshot injuries, and after amputations consequent upon them, together with its severe and obstinate character, often in men of previously sound constitutions, in military practice, contrasted with the comparative rarity of its occurrence in sound constitutions in civil practice. After the Crimean campaign, Dr. Valette, a French military surgeon, who had had one of the large hospitals at Constantinople under his charge during the period of the war, and again, since the Italian campaign of 1859, M. Jules Roux, the principal surgeon at the large marine hospital of St. Maudrier at Toulon, had both written at considerable length on the subject. Dr. Valette's observations were chiefly directed to this inflammation in its earlier and more acute stages, as witnessed amongst the wounded sent directly after the battles of Alma and Inkerman, amongst whom it had produced the most fatal consequences. The author remarks that in perusing Dr. Valette's reports, the conclusion could scarcely be avoided that the so-called osteo-myelitis, in a large number of the instances referred to, must have been truly cases of pyæmic poisoning, and that in all, the symptoms of the osteo-myelitis must have been greatly aggravated by circumstances tending to the development of pyæmia. Dr. Valette found all attempts to check the disease ineffectual, and came to the conclusion that all resections and amputations for the effects of this inflammation after gunshot fractures should be abandoned, and exarticulations substituted, the wounded being scattered at the same time in tents as widely as possible. M. Jules Roux's observations were made on the disease in its more chronic condition, and he was led to advocate the same views with regard to the necessity for exarticulation as had been advocated by Dr. Valette. M. Roux had under his care about 2000 soldiers who had been wounded in the Italian campaign, a considerable number of whom presented diseased conditions demanding con-

secutive amputation or other surgical interference. At first M. Roux practised amputation, but with such unfavourable results that he was induced to try exarticulation in similar cases instead. This operation proved remarkably successful. There was no death out of twenty-two successive cases, among which were four cases of exarticulation at the hip-joint. In a memoir on the subject which was read before the Imperial Academy of Medicine at Paris in 1860, M. Roux argued that when osteo-myelitis after gunshot wounds assumes a chronic form, amputation generally only takes away a portion of the inflamed bone, and in consequence of this incompleteness in the operation the disease is aggravated in the remainder. Hence, he asserted, the failures of secondary amputations for gunshot wounds of bones; and hence, also, in his opinion, the preference which ought to be given to exarticulation, or removal of the whole of the diseased bone, when a surgical operation becomes indispensable. The views of treatment propounded by M. Roux have led to several protracted discussions at the Academy of Medicine at Paris. They were particularly analysed in an elaborate discourse by Baron Larrey, which he afterwards published. In this discourse Baron Larrey arrived at certain conclusions, six in number, with the general terms of which the author believes that most English army surgeons would agree. The following are the conclusions referred to:—1st. Osteo-myelitis after gunshot wounds is more frequent than has been hitherto supposed; but is not inevitable, and in most instances is a means of cure. 2nd. It may either be limited to a given point of the bone, extend itself partially, or invade the whole of the bone more or less quickly. 3rd. Every rational mode of treatment must be adopted in the first instance. We are encouraged to do so because we know osteo-myelitis is susceptible of spontaneous cure. 4th. Sometimes it necessitates resection, and sometimes consecutive amputation, and sometimes, in certain cases, exarticulation is preferable. 5th. The existence of osteo-myelitis is sufficient to explain the want of success which occasionally follows partial operations in bones affected with this inflammation. But, 6th. It does not justify the two exclusive propositions in surgery, that resection of joints and amputations in the shafts of bones are to be abandoned for exarticulation in all such cases. The author of the present paper thinks, however, that the settlement of the question of the proper treatment of chronic osteo-myelitis might be carried a step further in precision, especially in cases where want of success had seemingly followed partial operations on account of its presence. Many cases, he states, come before military surgeons, in which, after amputation had been performed in continuity for gunshot injuries, or for the effects of osteo-myelitis consequent upon them, the portions of the limbs left afterwards presented such diseased conditions as to necessitate further surgical interference in order to avert fatal consequences from the patients. In these cases, where the morbid state of all the tissues is manifestly due to the continued osteo-myelitic action subsequent to the amputation, what is the course to be pursued? The arguments of M. Jules Roux would urge most forcibly in these cases, that exarticulation is the only treatment that can

scientifically be adopted; and even according to the conclusions of Baron Larrey these would appear to be the certain cases to which he refers in his 4th conclusion, where curative treatment in the first place and consecutive amputation in the next place having failed, disarticulation would be the preferable course to follow. The author has been led to adopt a different conclusion; and, in order to show the grounds on which his conclusions have been based, he calls attention, firstly, to certain preparations belonging to the Museum of the Army Medical Department from cases in which exarticulation had been performed or death had occurred on account of osteo-myelitis; and secondly, to the histories of some similar cases in which a cure had been effected without exarticulation being resorted to. The first three preparations exhibited consisted of the upper portions of three humeri. In each of these the history was—amputation at the middle of the upper arm for a gunshot wound, and exarticulation at the shoulder within a year afterwards for osteo-myelitis. The fourth preparation was one of the upper part of the femur which had been removed from the patient after death. Amputation had been performed in the middle of the thigh for a gunshot wound in India, and the patient died about a year afterwards from the effects of osteo-myelitis in the stump. There was every reason to believe that in all these cases the osteo-myelitis was due to the shock of the original gunshot injury, and not to any peculiarities in the amputation or other causes. To show that the simple shock of a gunshot wound is capable of giving rise to general endosteitis in a bone, another preparation was exhibited in which the entire shaft of the femur had been subjected to the action of this inflammation. In this case a musket ball had only penetrated the soft tissues and struck the bone, without producing complete or even a partial fracture of its substance. Another preparation of the upper half of a humerus was exhibited from a case in which the author had performed exarticulation for osteo-myelitis four years ago, before his attention had been turned to other modes of treatment. In this case the previous amputation had been performed for the effects of a kick from a horse, and the preparation was exhibited to show that the consequences of the osteo-myelitis were exactly similar to those which had occurred after the gunshot injuries. All the preparations above named showed that in each case extensive necrosis of the shaft had resulted from the endosteitis with which it had been affected; that the necrosed portions were well defined within fixed limits; that in no instance was the necrosis continued to the apophyses, although in all the cases the apophyses were more or less in the condition known by the term "osteoporosis;" and that the sequestered portions of the shafts were surrounded by copious shells of new bone, as in cases of ordinary necrosis. Three cases are then related in which amputation at the middle of the thigh had been followed by osteo-myelitis in the stump, but in which cures had been obtained without exarticulation. The amputation had been performed in two of these cases for gunshot wounds, in the third for the consequences of a compound fracture from a fall. In each of these cases the removal

of the sequestra left by the osteo-myelitic action was affected by surgical interference, and a sound and healthy condition of the stump resulted. In the case first described, the patient at the time of his admission into hospital at Fort Pitt, from India, had suffered so severely from the effects of the prolonged irritation to which he had been subjected, and the thigh-stump was so extensively diseased throughout, that at a consultation of the staff of the hospital the removal of the stump at the hip-joint was determined to be the only course which held out a fair hope for the patient's recovery. Fortunately, before this serious operation was undertaken, a study of the preparations laid before the Society, and some others of a similar kind, led the author of the paper to determine, as a preliminary measure, to open freely the cicatrix of the amputation-wound, and to take steps for removing all pieces of necrosed bone that might be found within the remaining portion of the shaft. The operation was so conducted that, if necessary, it could have been converted at the time into amputation at the hip-joint, or this formidable operation be reserved for a subsequent resource, if the removal of the necrosed bone did not lead to cure. Complete success, however, attended the first effort: the dead portion of the shaft, which reached up to the trochanters, was extracted, together with some smaller detached fragments. The patient rapidly improved in all respects afterwards, and eventually walked from the hospital with an artificial limb applied to the stump, which had become perfectly sound. The second and third cases mentioned were those of soldiers, who had suffered amputation of the thigh for gunshot wounds, and from subsequent endosteitic necrosis in the stump. In both cases the necrosed portions of the shaft were removed by gradual traction through openings in the line of cicatrix of the amputation-wound. In one of these instances, in which the man's limb had been smashed by a round shot just above the knee, at Lucknow, in 1857, an opportunity was afforded of examining the state of the stump five years after the date of the amputation. The stump was then thoroughly sound, and the man able to perform hard work and long journeys by wearing an artificial limb upon it. The motions of the hip-joint were perfect. The author of the paper states his present conviction to be, that if similar steps had been adopted, and the necrosed sequestra removed, in the instances brought before the Society in which exarticulation at the shoulder had been performed by himself and others, the stumps might have been similarly preserved; and that in the instances of the femoral stump, and the femur affected with endosteitic necrosis, the lives of the patients might probably have been saved by such a proceeding. In cases where amputation had been previously performed, the amputation cicatrix should be opened for the removal of sequestra, or, if more convenient, the stump could be opened from other directions; where no previous amputation had been done, the sequestra should be extricated as in ordinary cases of necrosis. Though not a matter of such importance to avoid exarticulation of a humeral stump as it is of a femoral stump, owing to the danger to life in the latter operation, and the important use of a thigh-stump

for the adaptation of mechanical contrivances for assisting in supporting the weight of the body, yet the author maintains the preservation of a humeral stump to be of great value to the possessor, especially when the power of compressing it to the side is retained. An osteo-porotic condition of the articulating heads of the bones, corresponding with the condition shown in the preparations, will not interfere with a successful result if the necrosed sequestra be completely removed. The author alludes to a case in which he had removed a foot at the ankle-joint, in which, on sawing off the two malleoli, the extremities of both the tibia and fibula were seen to be extensively affected with fatty osteo-porosis; yet the ends of these bones became firm and solidified under an improved condition of general health, the removal of the source of irritation which had previously existed in their immediate neighbourhood, and the stimulus of use. There could be no doubt that the head and neck of the femur in the case of the thigh-stump which had been preserved by the removal of the sequestra, the largest of which was exhibited to the Society, was in a state of osteo-porosis at the time these sequestra were extracted. The amount of irritation to which the bone had been subjected, the length of time that had elapsed, together with the conditions observed in analogous cases where the opportunity of examining the conditions had been afforded, sufficiently established the fact. The author concludes by observing, that while adopting generally the views of Baron Larrey, before quoted, in reference to the nature, progress, and treatment of osteomyelitis after gunshot injuries, the following appears to be fair deductions from the facts and observations he has brought to the notice of the Society:—1st. In gunshot injuries of bone, it will be found for the most part—what might be anticipated from the intimate connexion which exists between the periosteal and endosteal investments of the bony tissues, and from the violent general mischief effected by the stroke or passage through them of a projectile—that all the structures participate not only in the immediate local destruction, but also in the extended inflammation which follows, whether the inflammation after a time subsides, and terminates in repair, or whether it continues in a chronic form. 2nd. There exists this difference between the inflammation of the endosteum and that of the periosteum: that of the endosteum has a special tendency after gunshot injuries to degenerate into a chronic condition analogous to that of suppuration in other tissues, to extend itself along the cancellated structure, and thus to produce disintegration and death of the body substance; that of the periosteum, at the same time, will exist only to such a degree as to cause it to exert a protective influence by the formation of new bone around the diseased tissues, just as in ordinary cases of necrosis from other causes. 3rd. If amputation in continuity be performed while the endosteum is suffering from the inflammatory irritation excited by the violent injury to which the whole bone has been subjected, especially when this has assumed a chronic form, the endostitis will most probably still pursue its course, even though the divided soft parts may at first become healed, slowly inducing death, more or less

extensive, of bony tissue, and, in time, the usual consequences of such a condition throughout the whole stump. 4th. The morbid condition of the endosteum does not usually extend from the shafts of bones into their apophyses. 5th. When amputation has been followed by these consequences, exarticulation should not in any case be resorted to for the removal of the diseased stump until the effect of complete extraction of the dead bone by proper surgical interference has been ascertained. 6th. Experience shows that, even although a patient's constitution may be greatly impaired by the prolonged local diseased action to which it has been subjected; and though there may be every reason to conclude that the articular extremity of a bone is in the condition understood by the term "osteoporosis," yet the complete removal of the endostitic sequestra may speedily be followed by restoration of the general health, and by a condition of the stump so sound and firm that it may be applied to any purpose of utility for which, according to its length and position, it may be competent.

ART. 98.—*Traumatic Separation of Skin without Wound.*

By Dr. HELLER.

(*Ztschr. f. Wundärzte u. Geburtsh.* xvii., 2, 1864; and *Schmidt's Jahrbücher*, 124, 1864.)

CASE.—This case is that of a man aged 28 years, who fell from a scaffolding two stories high, struck his left loin against a board platform, and continued his fall to the ground. The patient complained of pain and soreness of the injured part; where there was no wound, but only a certain laxness and moveableness of the skin, extending from the last dorsal vertebra to the middle of the sacrum, and for a hand's breadth on either side of the median line. There could be no doubt that the skin was detached by the blow from the parts beneath. After a time, fluid was effused under the skin; and ten days after the injury a quantity of clear serum was evacuated by an exploring trocar. Pressure and iodine ointment were applied; and in a month after the injury the patient had entirely recovered.

ART. 99.—*On the Lithexère.*

By M. MAISONNEUVE, Surgeon to the Hôtel Dieu, Paris.

(*Journal of Practical Medicine and Surgery*, Jan., 1865.)

This instrument, the *lithexère*, is used for the extraction from the bladder of detritus and fragments of stone after the operation of lithotripsy.

Crushing of the concretions is but the first part of lithotripsy, and the removal of the fragments is often extremely difficult. Hitherto several varieties of catheter-forceps have been used for the purpose, and also copious injections of warm water, but angular fragments, despite these measures, not unfrequently become impacted in the urethra, and, while causing intolerable pain, form a serious impediment to the discharge of urine.

The object of M. Maissonneuve's *lithexère* is to supply the desideratum. The instrument is hollow, and presents the outward appearance of the common lithoclast or crushing forceps, and bears on the concave side of the extremity a wide aperture, into which the gravel and detritus spontaneously gravitate. The peculiar merit of the contrivance consists in the presence, within the tube, of a spiral screw, worked by a crank, which grinds the fragments to a fine powder, and expels them from the bladder.

This very simple appliance considerably shortens the duration of lithotritic procedures, and admits, at one sitting, of the crushing and extraction of a calculus weighing two-thirds of an ounce.

SECT. II.—SPECIAL QUESTIONS IN SURGERY.

(A) CONCERNING THE HEAD AND NECK.

ART. 100.—*Experimental Inquiries respecting certain Wounds of the Skull.*

By Mr. W. F. TEEVAN, Surgeon to the West London Hospital, &c.

(*Medico-Chirurgical Review*, July, 1864.)

The experiments, 125 in number, were made in the dead-house of Westminster Hospital on the skulls of persons ranging in age from 16 years to 60 years, within one month after their deaths. The instruments used were spherical bullets, conical bullets, pick-axes, crowbars, nails, and bricks.

The result of this inquiry is, that *when a foreign body passes completely through any part of the skull—it matters not what the direction may be—the aperture of exit is always larger than the aperture of entry.*

The explanation offered is this:—*The aperture of entry is caused by the penetrating body only, whilst the aperture of exit is larger than the aperture of entry inasmuch as it is made by the penetrating instrument plus the fragments of bone driven out of the proximal table and diploë.*

ART. 101.—*Case of Aneurism by Anastomosis of the Scalp treated successfully by Setons and Ligature of the Common Carotid.*

By Mr. GEORGE SOUTHAM, Surgeon to the Manchester Royal Infirmary.

(*Proceedings of the Royal Medico-Chirurgical Society*, March 14, 1865.)

CASE.—The patient, a married female, aged twenty-eight years, had been suffering from the disease for upwards of eight years. She became an in-patient of the Manchester Royal Infirmary in May, 1864. The temporal artery and its branches, with the exception of those distributed around the in-patient of the Manchester Royal Infirmary in May, 1864. The temporal artery and its branches, with the exception of those distributed around the

eye and forehead (which, though visibly distended, were not pulsatile), were much enlarged, some of them almost to the size of the little finger, and communicating to the hand a distinct arterial thrill. The occipital artery and its branches were also similarly affected, though in a less degree. Pulsation was only slightly checked by pressure on the trunks of these vessels, but was completely suspended by compression of the common carotid. An ulcer had formed over the parietal protuberance, which had bled rather freely on several occasions. An attempt was made to remove the lint from the ulcerated surface; but arterial hæmorrhage supervened to so great an extent that the bandages were immediately replaced. On the following day, having taken the precaution to have the requisite appliances for the arrest of hæmorrhage at hand, the compress was removed in the presence of several members of the hospital staff. Profuse hæmorrhage followed from the ulcerated surface, which occupied a space of about an inch and a half in diameter. Pressure with the fingers failed to stop the effusion of blood, escaping as it did from countless sponge-like orifices. Lint steeped in a solution of perchloride of iron was also applied, and the carotid compressed; but the blood continuing to flow, with the consent of his colleagues, Mr. Southam proceeded to place a ligature on the trunk of the common carotid, which had the desired effect. Seven days after the operation the vessels of the scalp were soft, flaccid, and apparently bloodless; but very feeble pulsation could be felt in the course of the temporal artery. Four setons of worsted, about four inches in length, were now passed through the vessels, one across the temporal fossa, the others through the parts of the scalp where the vessels were most distinct. The week following, some of the vessels near the original sore were found distended, and slightly pulsatile. Three setons were inserted through them, and another about a fortnight afterwards at the posterior part of the occiput, where a vessel about an inch and a half in length could be distinctly traced pulsating slightly. From this period the case proceeded satisfactorily, and on the 12th of August she left the hospital quite well, with the exception of a small ulcer at the back of the ear, which was the remains of a slough that had taken place in that part. At the end of December there were no signs of any return of the disease.

The author remarks that the success of the plan of treatment adopted in the above case affords a prospect of bringing this hitherto unmanageable complaint more effectually under the control of the surgeon. At the time of the patient's admission into the hospital, the disease had arrived at a stage when prompt measures were absolutely necessary for the preservation of life. Accordingly, on the supervention of hæmorrhage, ligature of the carotid was immediately resorted to. But the unfavourable results which have frequently followed deligation of the carotid for aneurism by anastomosis of the scalp induced the author not to rely solely on that method of treatment; and the further progress of the case showed that if other means in addition had not been employed, no permanent benefit would have resulted from the operation. This need excite no surprise, for the operation to be successful must either permanently cut off the circulation through the diseased vessels, or lead to their obliteration—conditions which, however probable when the disease is confined to a single vessel and assumes the ordinary form of aneurism, are not likely to follow when several are affected, as in the present case, involving the entire temporal system, with its arteries, veins, and capillaries. For the blood in the

vessels after deligation does not coagulate, but readily finds its way into the general circulation, and the vessels remaining unchanged become again distended as soon as the circulation through the anastomosing branches is re-established. But, despite these drawbacks, deligation, even when not required for the suppression of hæmorrhage, has its advantages; for the temporary interruption which it causes to the circulation through the diseased structures affords a favourable opportunity for the application of other remedies. Setons were therefore resorted to as soon as there were indications that the scalp was supplied with blood sufficient for reparative purposes. They were applied at intervals wherever any return of pulsation showed itself.

It is unnecessary to dwell on the futility of trusting to setons only in the treatment of these cases. They have so repeatedly failed that their success in the present instance must be attributed to the quiescent state of the circulation produced by the ligature of the carotid. In confirmation of this view, the author refers to the case of a young lady who was under his care several years ago, whose index finger and thumb had become, through enlargement of the vessels, twice the natural size. Some of the vessels were in circumference as large as goose-quills, and gave a livid-bluish appearance to the fingers. Not the least pulsation or arterial thrill could, however, be discovered; and the vessels could be partially emptied of their blood by pressure. Three fine worsted setons were passed from the hand to the apex of the finger. Others were inserted at intervals. At the end of six months all evidence of the disease had disappeared. Deligation of the arteries, therefore, prior to the insertion of setons, does not appear necessary in all cases of aneurism by anastomosis. The disease is an affection of arteries, veins, and capillaries, varying in its characters according to the extent to which each of these structures is implicated. Deligation, therefore, seems to be required where the arterial tissue is principally involved, or where the enlargement of the capillaries has taken place to such a degree as to enable the force of the heart's action to communicate its impulse through the capillaries to the blood circulating in the veins. Similar considerations will also determine the extent of deligation, which, except where severe hæmorrhage occurs, need, in the majority of cases, only be applied to the smaller arteries.

Though setons were employed in the case now related, yet galvanism, the injection of perchloride of iron, and other similar agents, may, in some instances perhaps, be advantageously substituted; and even the risks attending ligature of the arteries may, by instrumental or digital compression, be occasionally obviated.

ART. 102.—*A Successful Case of Paracentesis Capitis.*

By Mr. T. Y. THOMPSON, Newcastle-on-Tyne.

(*Proceedings of the Royal Med.-Chir. Society*, June 28, 1864.)

CASE.—An infant, born healthy, June 8th, 1861, two weeks afterwards sustained a fall, and three weeks later the head was visibly enlarging.

On September 25th, 1861, the head measured circularly, the tape being placed over the parietal eminences, 20 inches, and laterally, encircling the chin, 21 inches. The iodides and other absorbent remedies were given, and various external applications were made, but the distension continued to increase, and on May 1st, 1862, the circular measurement was 24½ inches, and the lateral 24½ inches.

On May 3rd, 1862, the operation of tapping was performed. A trocar about the size of an ordinary crow-quill was employed, and introduced through the coronal suture, on the left side, about 1½ inches from the anterior fontanelle. About ten ounces of a clear liquid, resembling water, were drawn off; the soft parts forming the vault gradually collapsing. The head was then encircled in broad strips of adhesive plaster.

First day, the liquid continues to ooze out, several ounces have thus escaped: slight febrile symptoms. Seventh day, fresh strips of plaster applied. Tenth day, a strong convulsion, contractions alternating with relaxations, limited to the right side. Two teeth ready to protrude, the gums punctured, and an aperient given. No return, after continuing two hours.

On June 9th, five weeks after the first operation, the distension being again considerable, the trocar was again introduced, and about four ounces of liquid withdrawn, milky, and of the consistence of weak gum-water. No untoward symptom occurred.

The head did not again enlarge. At the end of August, 1862, the soft parts forming the vault were still depressed. During 1863, the scalp became more and more firm. June 8th, 1864—The child is now three years old, strong and well nourished. The head measures, circularly, 23½ inches, and laterally 23 inches. The whole of the vault is firm. The mental faculties are in no way impaired; on the contrary, the child is quick and intelligent for its age. The teeth are all present.

The exact situation of the liquid could not with certainty be determined. It was thought to be in the arachnoid. The history of the case, the occurrence at first of a circumscribed swelling, becoming gradually diffused and finally followed by enlargement of the whole head, pointed to this supposition. The first operation must have excited some inflammation, which morbid process producing a liquid of greater consistence than that previously present, may have mechanically prevented the exudation of more liquid, or by exercising a certain amount of pressure caused that which was left to be absorbed.

ART. 103.—*Case of Gunshot Wound of the Cerebellum.*

By Dr. BATTWELL.

(*Philadelphia Medical and Surgical Reporter*, and *British Medical Journal*, March 11, 1865.)

CASE.—Lieutenant W. B., shot through the head on the previous day, was brought into hospital on September 2, 1864. He was perfectly unconscious, breathing stertorously, with a wound about one inch above the mastoid process, and presenting itself on the opposite side, where the cerebral matter left no doubt as to the fact that the brain was injured. Towards evening he became conscious, and very restless, complaining of loss of vision, and inability to raise his head. Towards midnight he commenced to scream, and for three weeks did so incessantly, even during his moments of sleep. Dr. Battwell removed him to a distance from the hospital, and on the 20th he

became *suddenly* composed and quiet, answering perfectly any questions asked him. The left side, for some days, seemed paralysed, as he could not use his hand or leg on that side as he did the hand or leg of the other side. The face was drawn to the *right*, and he seemed to have some difficulty in swallowing. The secretions were natural, and he never lost control over them. He had constant priapism, with seminal emissions. All these symptoms, however, became gradually better, but as soon as he became able to move about, he seemed unable to guide his movements; the power of *will* over motion seemed lost, and for some time he was obliged to be led by his nurse. Finally, this symptom became better, and he convalesced rapidly. The pupil of the left eye seemed permanently dilated and intolerant (?) of light. During the process of healing several spiculae of bone came away, but about the seventh week all discharge from the wound had ceased, and he was transferred to Nashville.

ART. 104.—*On Exophthalmic Goître.*

By M. PETER.

(*Gaz. Hebd. de Méd. et Chir.*, p. 180, 1864; and *Ophth. Rev.*, Jan., 1865.)

After mentioning that Basedow and Teissier have noticed increased temperature of the body in cases of this affection, the author quotes, as an example, a patient of Trousseau's, in whom the pulse is habitually 120, and the skin always hot and dry. During an exacerbation, the pulse rapidly mounts to 140 or 150, and the sensation of heat becomes intolerable. M. Trousseau found in this case a symptom not previously noticed—it is what is termed a cerebral spot (*tache cérébrale*)—i.e., when the epidermis is slightly irritated, a beautiful red spot appears within two seconds, and lasts for about a minute. There appears to be an asthenic condition of the vaso-motor nerves, owing to which the slightest irritation causes rapid dilatation of the capillaries, just as occurs in some cases of fever. The author thinks the three phenomena, quickness of pulse, increased heat, and cerebral spot, are of the same kind, depending on a serious lesion of the vaso-motor nerves.

The author then gives the following interesting case:—

CASE.—A woman, aged sixty, was admitted into the Hôtel Dieu on the 3rd of July, 1863, with exophthalmus. In 1856 she was deeply grieved by the loss of her father. In a single night, passed in weeping, she felt her eyes swell, the thyroid gland enlarge and beat, and the heart violently palpitate: during the same night she had a very severe epistaxis; four days later she consulted M. Desmarres. A year later she went to Africa, where she was attacked by intermittent fever: for this she was admitted into hospital, and whilst there the goitre disappeared. Some time after the commencement of the exophthalmus, she suffered for more than a year from extraordinary hunger, so that she was obliged to eat almost every second hour; at the same time there was a tolerably severe diarrhoea. She was menstruating when the disease commenced; the catamenia stopped on the same night, and have not again appeared. In January, 1863, there was an attack of angina pectoris. On her admittance there was considerable exophthalmus: the free border of the lower lid, instead of forming a tangent to the cornea, was more than four millimetres from it; the upper lid, which

usually covers a portion of the cornea, was more than two millimetres from it. She was presbyopic, using convex glasses in reading or sewing; she could not bear a strong light. The heart beat forcibly; it measured thirteen centimetres longitudinally, and twelve centimetres transversely; there was no souffle either at the base or apex, nor was there one in the vessels of the neck; the pulse was 96. There was no goitre.

On the 9th December she had an apoplectic attack, which ended in death after twenty-four hours.

Post-mortem Examination.—A large extravasation in the left hemisphere, near the corpus striatum and thalamus opticus; a very small one in the pons. Heart very large; wall of the left ventricle especially thickened. Free edges of the mitral valve thickened, but no insufficiency or contraction; aortic valves a little rough along their free borders, but no insufficiency. Calcareous and atheromatous deposits in the aorta. No appreciable change in the vessels at the base of the brain; the capillaries around the extravasation showed no disease on microscopical examination. Spleen large; capsule not thickened, tissue firm, Malpighian bodies hypertrophied. Cirrhosis of the liver; fibrous capsule thickened, &c. In the kidneys, traces of interstitial nephritis. Thyroid small; the lobes hard, having a lobular appearance, owing to the retraction of their fibrous tissue; arteries small, presenting no sign of disease. The eyes pressed out of the orbits by cellular and fatty tissue, which almost entirely fills the orbital cavities; it is a little redder than usual. The ophthalmic artery appears normal. The globes present no change. The bones of the skull were extremely vascular, and more than double their ordinary thickness. Upper and middle cervical ganglia normal; inferior increased in size and reddened; on microscopical examination, increase of the connective, and diminution of the nervous elements. No decided change of the cardiac plexus.

The author remarks, that congestion alone could account for the sudden appearance of the symptoms in this case, a supposition confirmed by the epistaxis. He then points out that continued congestion may induce hæmorrhage, serous discharges, inflammation, or hypertrophy, and, by secondary contraction of hypertrophied fibrous tissue, cirrhosis. All these results were present in this case, repeated bleeding from the nose, and diarrhoea, atrophy from cirrhosis of the thyroid gland, commencing cirrhosis of the liver, hypertrophy of the heart, hypertrophy with hyperæmia of the orbital cellular tissue, hypertrophy of the cranial bones. According to him, this case justifies us in believing that the various functional derangements present in exophthalmic goitre depend on temporary congestion or organic disease of the sympathetic system; either of these conditions becoming the cause of temporary congestion or permanent lesion in different organs, as already explained.

ART. 105.—*On Vascular Protrusion of the Eyeball.*

By MR. NUNNELEY.

(*Proceedings of the Royal Med.-Chir. Society*, Nov. 22, 1864.)

This paper, which is a sequel to a paper noticed in a former volume (xxx. p. 184), is based on a second series of three cases and two post-mortem examinations of so-called aneurism by anastomosis of the orbit. It is suggested that vascular protrusion of the eyeball is a more correct name for cases of this kind than the name commonly used, namely, aneurism by anastomosis of the orbit.

The first of this second series of cases was of traumatic origin; it occurred in a man who was thrown from his horse. There were very decided symptoms of fracture of the base of the skull. In a few days after the accident, the left eyeball began to protrude and to exhibit all the symptoms characteristic of the disease. Five weeks after the accident, as the patient was getting worse, and there was fear of the eyeball sloughing, the common carotid artery of the same side was tied. The patient speedily and completely recovered, and now (eighteen months after the operation) remains well.

The second case was of spontaneous origin, in a woman aged forty-seven. It occurred suddenly, soon after she got out of bed. It had existed ten months. Beyond rest and cold lotions, very little treatment had been submitted to. It occasioned considerable distress, and incapacitated her from much work, as all exertion increased the suffering. It was probable that, before long, she would have to submit to more active means.

The third case was also spontaneous in its origin. For a long time the true cause of it was very obscure. The protrusion of the globe was excessive. Although ligature of the carotid (which was resorted to) was successful in arresting the protrusion of the eye, owing to other tumours which the patient suffered from, his health continued to decline; and he died, exhausted, eighteen months after the operation. The eyeball had collapsed, and had given no further trouble. For some time before the patient's death the disease was known to be malignant. A post-mortem examination revealed a tumour developed in the cavernous sinus, pressing upon the ophthalmic vein, and passing into the orbit and the zygomatic fossa. Another tumour, which also during life had been pulsating, passed through an opening in the right parietal bone on to the brain. A large tumour projected externally from the sternum, and also into the cavity of the chest. The thyroid was a large mass of malignant degeneration.

The second post-mortem examination was of the body of Mrs. J—, whose case was reported in the first series of cases. She died upwards of five years after the operation had been performed, since which, until a few days before her death, she had generally been in good health. The fatal disorder was thought to be acute bronchitis; she suddenly became comatose, and died soon afterwards. The entire brain was found to be small; and the anterior lobe of the cerebrum, on the side in which the carotid had been tied, was considerably smaller than that on the other side. This condition, however, the author thought could hardly be attributed to the ligature of the vessel, for which he gave his reasons. On the side of the sella Turcica, just as the ophthalmic artery was given off from the carotid, was found a circumscribed aneurism, filled with a solid coagulum, which pressed upon the ophthalmic vein, and thus occasioned the protrusion of the eyeball.

The author thinks these seven cases, and the three post-mortem examinations, satisfactorily show the true cause of the ocular protrusion to be pressure upon the post-ocular veins, which interfered with the free return of blood from the eye and orbit. He contends that usually there was no disease whatever in the orbit itself, the

condition of the parts in it being merely passive, as a swollen limb is below a large popliteal or axillary aneurism; and that though a circumscribed aneurism, as in most sudden cases of spontaneous origin, or a diffused aneurism from rupture of a bloodvessel in traumatic cases, was the most common cause of the development of the affection, this was by no means a necessary condition for the existence of it. He believes that it might be induced by anything which causes pressure upon the ophthalmic veins, or difficulty in the return of blood from them. Though in the most acute form the disease had been rarely seen, and its nature had been misunderstood, the author was inclined to think that in more chronic and much subdued degrees it was not so very uncommon. He considers that in many cases of protruded eyeballs in weak and delicate persons, in those with bronchocele or cervical tumours, and where there is impeded circulation from disease of the heart (particularly of the right cavities), or of the lungs, the disease is essentially of the same character as the more acute affection, though at first sight they may appear to be very different, and certainly do require very different management.

ART. 106.—On the Comparative Value of Iridectomy and of Hancock's Operation.

By M. WECKER.

(*Journal of Practical Medicine and Surgery*, Jan., 1865.)

Mr. Hancock, in his operation, inserts the knife at the lower and outer part of the margin of the cornea, in an oblique direction downward, so as to divide the fibres of the sclerotic to an extent of about two lines.

The following parts are thus cut through:—1, the bevelled border of the sclerotic; 2, Schlemm's canal; 3, the ligamentous attachment of the iris; 4, the knife then crosses the lower part of the anterior chamber; 5, the circular fibres of the ciliary muscle, which rest on the internal wall of Schlemm's canal, are divided; 6, the knife gliding between the radiated fibres of the same muscle, penetrates into the corpus ciliare; and, 7, perforates the hyaloid, thus effecting an entrance into the vitreous humour. Mr. Hancock's section includes, therefore, a small extent of the sclerotic, gives issue to the aqueous humour, divides a few of the circular fibres of the ciliary muscle, and promotes the escape of a trifling amount of the vitreous humour.

The procedure is an association of two measures which have long been in practice for the relief of glaucoma—viz., paracentesis of the anterior chamber, and of the vitreous humour. In addition, a few fibres of the ciliary muscle are also cut through.

The efficacy of the double paracentesis in glaucoma is fully admitted by all oculists, and Mr. Hancock's method is in this respect advantageous. But it is not possible to concede to the evacuation any permanent beneficial results, or any favourable influence on the prevention of relapse. It is therefore proper to inquire whether the partial tenotomy of the ciliary muscle, superadded by Mr. Hancock, imparts to the procedure a more enduring character of

utility. M. Wecker does not think so, and he remarks that all the surgeons who have practically tested the respective value of iridectomy and of ciliary tenotomy, agree that the effects of each are different. In a disease of the nature of glaucoma, the partial section of a muscle, which in cases attended with acute inflammatory action is so promptly paralysed in consequence of the pressure exercised on its motor nerves, can have but unimportant results. The authors who ascribe to spasmodic irritation of the ciliary muscle an important share in the production of glaucoma, and to the section of its fibres a considerable degree of efficacy in the treatment of the disease, obviously lose sight of the all-important fact, that the spasm referred to is utterly neutralized by the paralysis of the motor nerves of the ciliary muscle, a fact abundantly demonstrated in glaucoma by the mydriasis and paralysis of accommodation always present in such cases.

In conclusion, Mr. Hancock's procedure is extremely useful in glaucoma as a means of relieving the inflammatory symptoms; but the section of a few fibres of the ciliary muscle affords no additional security against relapse, nor does it check in an absolute manner the fatal progress of the disease. These results are attainable by iridectomy alone. A liberal excision of the iris from its pupillary margin to its outer border, the removal of a considerable number of the terminal filaments of the ciliary nerves, and the incision of the sclerotic and iridian attachments, restrain the irritation of the ciliary nerves and any further increase of the secretions, diminish the distension of the eyeball, and the subsequent pressure on the ocular origin of the optic nerve and of its expansion, the retina. Recent researches have established the fact, that the secretion of the humours is mainly governed by the ciliary nerves, and that any protracted irritation of these filaments must necessarily increase the amount of the liquids included in the eyeball, and add proportionately to the pressure suffered by the envelopes.

M. Wecker further contends that glaucoma is not, properly speaking, a morbid entity, but a complication, or a symptom of inflammation of the deep-seated membranes of the eye, whenever the membranes, and more especially the sclerotic, are so unyielding, that moderate increase of the internal pressure is sufficient to occasion an amount of irritation and dragging of the ciliary nerves calculated to keep up and increase the hypersecretion which has already occurred within the organ.

M. Wecker is therefore of opinion that the spasm of the ciliary muscle and the section of its fibres, as performed by Mr. Hancock, can have no permanent influence on the symptoms of glaucoma. The favour with which the operation has been received by many surgeons is due to the fact that it is of more easy performance than iridectomy, and also to the relief which follows paracentesis. It is, indeed, much to be desired that surgeons were in possession of some more simple method than excision of the iris, but the much-required desideratum has not yet been supplied, and we are still compelled to acknowledge that iridectomy is the only curative proceeding in glaucoma, and the only method which surgeons can conscientiously resort to for the treatment of this formidable affection.

ART. 107.—*On the Radical Cure of Strabismus.*

By Mr. M. H. COOPER.

(Dublin Quarterly Review of Med. and Surg., vol. xxxv., p. 1, 1854.)

"This effusion," says Mr. Cooper—"is so formidable to the surgeon to deal with, and so fatal to the usefulness of the eye—racks with marvellous regularity to repeated weak injections. The inflamed and ulcerated conjunctiva is punctured, or scraped with the scissors if necessary, a careful student can be put beside the patient's bed, and shown how to send the contents of the syringe underneath the upper lid, from the external canthus across the eyeball. In the most acute cases a solution of a quarter of a grain of nitrate of silver to the ounce of distilled water should be used every ten minutes, for the first hour; after that a half-grain solution should be injected every half-hour. If this is carefully carried out for the first twenty-four hours, the patient's eye will be quite safe. A stronger solution may then be used; and, if needful, it may be followed in a couple of days by Guthrie's ointment of nitrate of silver, if the vicious condition of the conjunctiva should seem to require it. I have followed this plan of treatment, generally, for at least nine years; and in that time I have never lost an eye from gonorrhoea, ophthalmia, with one exception; in that case, the pupil in charge broke the syringe, and, thinking it a matter of no importance, waited for twenty-four hours to get it replaced: by this time the cornea had sloughed in one point, and the iris protruded. The man, however, was so fortunate as to recover, with comparatively slight injury to sight."

ART. 108.—*On the Radical Cure of Extreme Divergent Strabismus.*

By Mr. J. Vose SOLOMON, Surgeon to the Birmingham and Midland Eye Hospital.

(Pamphlet, London, Richards, 1864.)

In this pamphlet Mr. Solomon confines himself to the consideration of instances where the deformity has had its origin either in a too free division of the tendon of the inner rectus muscle and the sub-conjunctival fascia, by Dieffenbach's operation for squint, or in paralysis of the inner rectus, unconnected with tumour of the orbit.

The indications to be fulfilled by any proceeding undertaken for the radical cure of *extreme* divergent strabismus, when a result of previous tenotomy of the inner rectus, are:—To give the eye a central position; to restore the length and the proper point of attachment to the adductor muscle, thereby insuring balance of the opposed muscular forces and *permitting normal convergence*.

The division of the attachments of the inner rectus, with its conjunctiva and subjacent fascia (in a manner to be presently described), and of the tendon of the external rectus, suffices to place the eyeball in the axis of the orbit.

The great difficulty consists in protecting and fixing the globe until the inner rectus obtains a proper attachment to it. Careful

study of the subject led Mr. Solomon to institute in persons who are in good health the following plan.

(1st Step.)—Anæsthesia having been induced by chloroform, and the lids separated widely by a self-acting speculum, the first step of the operation is commenced by making a flat of the parts which contain the inner rectus. To effect this a rather long perpendicular incision is made at about half a line from the inner margin of the cornea, down to the sclerotica; with curved scissors the inner side of that membrane is dissected bare. In order to make the flap come forward and over the cornea, two short incisions are practised through the extremities of the first incision, towards the nose.

(2nd Step.)—As soon as it is found that the flap will reach, on being stretched, the vertical diameter of the cornea—when the eyeball is in the axis of the orbit—the external rectus is divided by a rather long perpendicular incision, just behind the insertion of its tendon.

The conjunctiva, which intervenes between this incision and the outer margin of the cornea, is reflected forwards, *and made to form a second flap.*

(3rd Step.)—The two flaps are united over the vertical meridian of the cornea by sutures, three of which are generally sufficient, one in the centre, one just above and one below the upper and lower margin of the cornea.

The eye now occupies the axis of the orbit; the inner rectus is stretched to its utmost limit, and the tendon placed over the most favourable site for adhesion to the sclerotica; the external rectus retracts, and if the power of moving the globe inwards and outwards be tested at this stage, it will be found still to exist, but to a slight degree only, the antagonism being about equal.

The *after treatment* aims at securing the immediate union of the inner flap, and the preservation of the nutrition of the globe. These indications are fulfilled by keeping both eyes closed, and with the temples covered for about ten days by jewellers' cotton wool. The central suture is removed on the second or third day so as to obviate *corneitis*. Inflammatory chemosis, should it occur, must be met by the application of two, three, or four leeches to the temple, and pain in the eye by the internal use of morphia. If there be much purulent discharge the local use of astringents is necessary. At the end of three weeks the conjunctiva should be trimmed of any uneven processes.

Mr. Solomon gives a case in illustration, and says that he has practised this method for upwards of four years with great success.

ART. 109.—*A Peculiar Method of Operating in Cases of Cataract and Synechia Annularis.*

By Mr. J. VOSE SOLOMON, Surgeon to the Birmingham and Midland Eye Hospital.

(*Ophthalmic Review*, April, 1865.)

In cases of this kind, Dr. Gräfe's proceeding is generally adopted in this country, and with not very favourable results; and therefore

this case, the operation performed, and the remarks upon it, are deserving of attention.

CASE.—A tall, powerful man, aged fifty-five, by trade a combmaker, but for some time an inmate of the Warwick union workhouse, was sent to me, as a patient of the Eye Hospital, by my friend, Mr. William Bodington, of Kenilworth, on the 12th of August, 1864. In consequence of ankylosis of the cervical vertebrae, his neck was rigid, and bent to one side; which, with great prominence of the eyes, and a fixed amaurotic stare, gave a most ungainly gait to the patient, and a peculiar expression to the face. His *left* eye had been injured eight years ago: it was blind, disorganized, and staphylomatous from past choroido-scleritis. The right eye had been diseased a year only. It presented very advanced tissual changes from sympathetic irido-choroiditis; the cornea was bright, and surrounded by a large ciliary venous zone; the iris, which was bossulated throughout, nearly touched the arch of the cornea. The whole circumference of that membrane presented numerous and complex anterior synechiae. The pupil was closed and adherent to the lens capsule. The vision was limited to a perception of shadows, which was more vivid than I have ever before met with. The patient, who had been addicted in his younger days to pugilism (which probably gave rise to the cervical ankylosis), was in excellent health.

Operations, August 15th.—The disorganized eye was enucleated; and a narrow bit of iris was taken by the canular forceps from the right, in order to ascertain the condition of the lens, which it was believed might perchance be transparent, as his perception of shadows was unusually good. The lens proved to be opaque.

Operation, August 27th.—The eye having recovered, the lens was extracted to-day by the lower section, the knife being carried through the sclera, and behind the iris. The cataract was easily removed, and without loss of vitreous, by a hook. It was a dark amber lens. A beautiful black pupil, with vitreous distending it, left nothing to be desired by the operator. The eye was therefore quickly closed, and dressed with jewellers' cotton wool, supported by a few turns of a roller.

Sept. 7th.—The wound is cicatrised, the pupil is well defined and open; but behind it are shreds of capsule, some soft lens matter, and lymph. The vision remains as before treatment. Tension less than normal (—Tr).

Sept. 21st.—The anterior chamber, from recession and flattening of the iris, is enlarged; the inference is that a communication subsists between the two aqueous chambers, although the fundus cannot be illuminated.—Operation.—A broad cutting needle was inserted through the upper circle of the ciliary sclera, and brought out in front of the iris, when with a sharp backward stroke the membrane was severed, and a good pupil, distended by vitreous, was obtained.

Sept. 27th.—There is still a further improvement in the dimensions of the anterior chamber and pupil, but vision is not commensurately advantaged.

October 7th; operation.—The pupil is drawn downward and closed, the upper border being well defined. The iris has lost its fibrillous character and become lighter in colour. It was therefore determined to make an artificial pupil, with a penetrating crotchet iris-hook (Bowman's). A small puncture having been made slowly through the upper corneal circle without loss of aqueous, the hook was introduced, and an equilateral triangular pupil was made, the apex of which became united to the point of puncture. The base measured about two lines.

November 22nd.—The fundus can now be, for the first time, focused. The limits of the optic nerve are ill defined; there is gray atrophy. The tension of the globe is normal, chronic enlargement of the ciliary veins still exists, but there is no evidence of irritation from the surgical treatment.

With a three-inch convex glass, features are distinctly seen at nine feet ; at the same distance he counts fingers, but they cease to be visible when separated to a greater extent than nine inches, so much contracted is the visual field. The poor man's inability to rotate the head, in consequence of the anchylosed condition of the spine, and his amblyopic and contracted field of vision, render fixation very slow.

Commenting on this case, Mr. Solomon says :—

"I am induced to report this case, from the very unfavourable results which have generally attended, in English subjects, the extraction of cataract, or the formation of an artificial pupil, where the same tissual changes have been present as were observed in the instance here given.

"Having satisfied myself, by examination, that the vitreous was, in all probability, of good consistence, it occurred to me that liability to ophthalmitis, or shrivelling of the corneal flap, might be lessened, provided, in the performance of extraction, I abstained from interference with the disorganized iris, and made my section through the sclera. In considering the position of such section, it must be remembered that the iris was bossulated and united near to its periphery by numerous morbid adhesions to the cornea—conditions which permitted a wound to be made much nearer to the sclero-corneal union than would have been practicable in the healthy eye. The passage of the knife between the front of the lens and the posterior surface of the iris, by separating intimate adhesions and opening the capsule, must have materially assisted the extraction of the cataract, and so promoted early union of the flap. Bowman's hook being of small size, and furnished with a sharp-cutting inner edge, affords a most useful instrument in the cases under consideration. It may be thrust through any selected point of the iris ; its cutting inner edge is well adapted for the division of the pigmented membrane which is frequently found behind the inner ring of the diaphragm, forming a bond of union with the lens capsule.

"The advantages of making the section through the posterior aqueous chamber (a method practised by Whyte,* in 1801, for the extraction of cataract, uncomplicated by other intra-ocular disease) appear in cases of annular synechia to be as follows :—(1), An easy release of the lens from its adhesions ; (2), the formation of a flap, edged with a membrane (the sclera), more favourably placed for nutrition than the cornea ; (3), a diminished liability to destructive irido-cyclitis, by non-interference with the diseased iris until the eye has recovered from the cataract operation.

"The theoretical objection to scleral section consists in the greater probability of loss of vitreous. As, however, it is scarcely possible to obtain less success than has hitherto attended the plan of performing iridectomy and extraction on the same day, I feel justified in suggesting to the profession a trial of the method here described, with a view of determining its value, as compared with other plans of treatment."

* Vide *London Med. and Phys. Journal*, 1802, vol. vii. p. 209.

ART. 110.—*On a Peculiar Affection of the Retina.*

By Dr. A. NAGEL.

(Centralblatt f. d. Med. Wissensch., No. 45, 1864; and Ophthalmic Review, Jan., 1865.)

The author found the following condition of both eyes in a young man of about 20:—"The whole of the retinal arteries, from their entrance at the disk up to their minutest branches, appeared as *white strings*, which were sometimes glittering and clearly defined, sometimes of a duller hue and with somewhat indistinct margins. A fine bright-red line was seen in the middle of the larger strings. Here and there this red line became somewhat larger, and where the white hue of the stripe was but little intense, the double contour of the artery could be traced, though indistinctly, as if through a haze. A very few small arterial twigs appeared normal, except that their red colour was somewhat dulled. The venous system also participated in the disease; the larger trunks were somewhat diminished and irregular in size, and a few of their peripheral branches were changed into white strings. The degeneration generally stopped at the junction of branches, so that the vein appeared formed by two twigs, one of which was red, the other, with all its ramifications, white. Some parts of the retina were a little clouded, untransparent; here and there a kind of network was formed by little whitish stripes, which were supposed to be small vessels with thickened walls. There were numerous punctated, and a few larger ecchymoses, partially collected into groups. White, translucent, prominent masses concealed the entrance of the central vessels on the optic disk. A great quantity of closely-packed red points and streaks in this situation proved to be formed by vessels whose origin and growth were watched for a long time. On slight pressure of the globe, every trace of red vessels disappeared from the white glittering deposit.

"Contrary to all expectation, his central vision was nearly normal. The visual field, however, was defective; at first the defect was island-like near the point of fixation; at a later period, by gradual increase, it formed an irregular zone, which inclosed a tolerably circular, slightly excentric field of vision, and was itself surrounded by a sentient portion.

"It was learnt from the history that the disease had progressed very gradually, and that at different times there had been brief obscurations of one or both eyes. The author thinks that such sudden, temporary obscurations—not unjustly denominated *epilepsia retinæ*—depend on momentary anæmia of the retina.

"The general health is unaffected, and the heart healthy; there is some reason to suspect a commencing central lesion, though, after watching the case for many months, the author has failed to discover any corroborative symptoms.

"This, although a solitary case, differs so essentially from the forms of retinitis hitherto described, and is at the same time so well

characterized, that the author has no hesitation in adducing it as an example of a special form of retinal disease. He thinks it must be considered to be a chronic, though slowly progressive inflammation, which commences in the walls of the vessels as a proliferation or induration of the connective tissue, spreads to the same tissue in the parenchyma of the retina, is transmitted from the arteries by means of the capillaries to the veins, and is possibly connected with similar changes in the central organs and their vessels. The latter suspicion is supported by the occurrence of the disease in both eyes.

"Wedl, Billroth, Rindfleisch, and Leidesdorf have shown that many diseases of the brain and spinal cord arise from the walls of the vessels. The author compares his case with one described by Rindfleisch, in which there were many centres of gray degeneration in the brain and anterior portions of the spinal cord, and in which the degeneration had evidently commenced in a proliferation of the cells and nuclei of the external coat of the vessels. The author thinks that the process was similar in his case, and that he might justly denominate the affection *gray degeneration of the retina*."

ART. 111.—*Loss and Recovery of nearly Perfect Vision
after Serious Injury to the Eye.*

By Mr. J. WILLIAMS.

(*Dublin Quarterly Journal of the Medical Sciences*, Nov., 1864.)

CASE.—Michael Haines, aged forty-six, sustained a severe injury of the right eye in December, 1861, from a fall on the handle of an iron plough. High inflammation ensued; when, after some days, he could open the eye, he found he was perfectly blind, and some weeks elapsed before he "could discern day from night." Gradually the vision improved, but "for a long time everything he looked at appeared red," and he was unable to distinguish the natural colour of objects.

May 22nd, 1863.—The eyeball is misshapen, but full and elastic to the touch; the cornea is clear, save where it presents two whitish lines, which do not interfere with vision; no iris can be seen; at the inferior part of the eye, towards the inner angle, and about three lines from the cornea, there is a fistulous opening in the sclerotic, through which protrudes a piece of vitreous humour, about the size of a pin's head. There is an external strabismus of this eye. A lighted candle held before the eye causes but one erect image. He can see with this eye, without the aid of a lens, a single hair, pin, or other small object, and also the smoke issuing from a chimney 500 yards away. He is not dazzled by the brightest sunlight, and can use the eye with the same facility as if the iris were present.

ART. 112.—*On a New Mode of Applying Atropine for Ophthalmic Purposes.*

By Dr. J. HOMBERGER.

(*Amer. Rev. of Ophthalm.*, 1864; and *Ophth. Rev.*, Jan., 1865.)

In place of repeated applications of a solution, the author recommends its use in substance. Solid atropine readily adheres to the end of a silver probe, and a particle may be thus placed on the inner surface of the lower lid. According to the author, the alkaloid soon dissolves in the tears, and does not excite any irritation. Two applications a day appear to him to have as much effect as twenty instillations. In iritis he introduces "the fortieth part of a grain of atropine, in substance, into the lower conjunctival sac, which can be easily done by placing the salt, with a probe, on the everted lower lid. The patient is kept for half an hour under observation. Dryness in the throat is a usual effect of the application of the drug, which soon passes away; only if further symptoms (congestion to the head, paralysis of the m. protractor urinæ) should approach, it will be necessary to give the patient, internally, one-sixth to one-third of a grain, or a subcutaneous injection of one-eighth to one-fourth of a grain of the sulphate of morphia. Though I have but twice been obliged to resort to these means of counteraction, I consider it necessary to have them always on hand. It will be well to examine the patient some hours after the first application. If the pupil has enlarged considerably, one application daily will soon bring about dilatation, and no further treatment will be necessary, particularly in cases of a non-specific nature. If the enlargement is noticeable, but of little extent, or if there is no change, another application is made with the same care, and the case re-examined the following day. On the second day, those cases which do not present a marked increase of the size of the pupil are, according to the current rules, subjected to the action of mercury, to depletion, paracentesis of the anterior chamber, or iridectomy. Those, on the contrary, where the pupil has become larger, are treated with atropine exclusively, and only those where marked constitutional syphilis exists, submitted to a mild mercurial treatment."

ART. 113.—*On the Extraction of Foreign Substances from the Ear.*

By Dr. E. BESSIÈRES D'EGREVILLE.

(*Journal of Practical Medicine and Surgery*, Dec., 1864.)

Dr. E. Bessièrès d'Egreville states that if the *curette* of Leroy d'Etiolles is not at hand, foreign bodies accidentally introduced into the ear may in general be readily extracted with a pin securely held between the blades of a forceps.

The author describes as follows, in the *Gazette des Hôpitaux*, the very simple plan he has several times resorted to with entire success :—

“The pin being securely fixed between the blades of a torsion-forceps, I bend its extremity in an extent of a line or a line and a half, and blunt the point by friction over some hard substance to avoid wounding the patient in the event of any sudden movement during the extraction. The pinna of the ear being placed in the position best adapted for the inspection of the duct, the pin is introduced between the lower edge of the obstructing body and the wall of the passage ; by a slight movement of rotation the hook is brought to bear on the foreign substance and easily draws it out. I have thus succeeded in extracting from the ear of a child aged four years, a smooth piece of stone firmly fixed in the passage, after several attempts had previously been made to remove it, without any other result than that of producing a considerable degree of local inflammation.”

The plan proposed by Dr. Bessières is doubtless an ingenious one, but cannot compare for efficacy with the strong injection of tepid water which we have often seen used with immediate success by various persons.

ART. 114.—*Excision of the Tongue.*

By MR. SYME, Surgeon-in-Ordinary to the Queen in Scotland,
Professor of Clinical Surgery in the University of Edinburgh,
Senior Surgeon of the Royal Infirmary.

(*Lancet*, Feb. 4, 1865.)

CASE.—In the early part of November last, Mr. W——, aged fifty-two, from Manchester, applied to me on account of a very formidable morbid condition affecting his tongue. From its point to the root it was swollen and indurated, the surface being of a brown colour and roughly tuberculated, so as to resemble the back of a toad. It was also nearly quite immovable, and, from completely filling the mouth, not only prevented articulation, but rendered deglutition impossible with respect to solids, and extremely difficult in regard to fluids. From the same state of matters, there was a most offensive fetor, through mucus secreted by the unhealthy surface not being permitted to escape.

The patient informed me in writing that he had suffered from uneasiness in his tongue for many years, but that neither articulation nor deglutition was seriously affected until 1862, since which time he had been under medical treatment in London as well as Manchester without experiencing any benefit. As palliation seemed all that could be expected, I offered some suggestions with this view, and advised that no time should be lost in returning home. But soon after his arrival there I began to receive from the patient very painful letters, reporting aggravation of the symptoms, especially in regard to deglutition, so that death from starvation seemed imminent, and urgently desiring some means of relief. To these appeals I replied that the only effectual remedy was removal of the tongue, and that this could not be done without very serious danger to life, so that the operation promised

nothing more than a chance of escape. This slight encouragement brought the patient back, and he arrived here on the 27th of December.

Being thus as it were compelled to make another trial of excision, I carefully considered all the circumstances concerned that might tend to interfere with its successful performance. Of these the one which most prominently presented itself was the prevention of voluntary deglutition that must result from depriving the os hyoides of the power by which it is drawn forwards. In the common cases of cut-throat, where a large transverse wound is made into the pharynx, although the suicide rarely accomplishes his object in the first instance, he still more rarely escapes the fatal effect of pulmonary inflammation induced by irritation propagated from the larynx ; and I did not forget that both the patients on whom I had performed the operation in question died from purulent effusion into the lungs. Instead, therefore, of cutting through all the muscles of the os hyoides, as had been done in the former cases, I resolved to retain the mylo-hyoidei and genio-hyoidei entire, and divide merely the attachments of the genio-hyoglossi. I also thought that it would be better to perform the operation without chloroform, since the patient, instead of lying horizontally, might thus be seated on a chair, so as to let the blood run out of his mouth and not pass backwards into the pharynx.

The operation was performed on the 29th, with the assistance of Mr. Annandale, Dr. Sewell, and Mr. Cheyne, to the first of whom I am especially indebted for his able co-operation. Having extracted one of the front incisors, I cut through the middle of the lip and continued the incision down to the os hyoides, then sawed through the jaw in the same line, and, insinuating my finger under the tongue as a guide to the knife, divided the mucous lining of the mouth, together with the attachment of the genio-hyoglossi. While the two halves of the bone were held apart I dissected backwards, and cut through the hyoglossi along with the mucous membrane covering them, so as to allow the tongue to be pulled forward and bring into view the situation of the lingual arteries, which were cut and tied, first on one side and then on the other. The process might now have been at once completed had I not feared that the epiglottis might be implicated in the disease, which extended beyond the reach of my finger, and thus suffer injury from the knife if used without a guide. I therefore cut away about two-thirds of the tongue, and then, being able to reach the os hyoides with my finger, retained it there while the remaining attachments were divided by the knife in my other hand close to the bone. Some small arterial branches having been tied, the edges of the wound were brought together and retained by silver sutures, except at the lowest part, where the ligatures were allowed to maintain a drain for the discharge of fluids from the cavity.

Next day I visited the patient, and finding him in all respects comfortable, inquired if he could swallow. In reply, he pointed to a drinking cup containing milk, and intimated that he wished it to be filled ; then, placing the spout between his lips, while his head was bent backwards, he drank the whole without any cough or sputtering. Having seen this I felt assured that the result would be satisfactory, and was not disappointed, as everything went on well afterwards. The only inconvenience experienced was from the edges of the jaw being occasionally displaced ; but this was easily remedied by an ingenious contrivance of Mr. Wilson, the dentist, who, finding that a silver cap inclosing the teeth was not sufficient for the purpose, fashioned a shield of gutta-percha, embracing the chin on each side, and secured to the metal plate by a wire.

Under an ample supply of nourishment by milk, soup, and soft solid food,

there was a rapid return of strength, so that an improvement in this respect was almost daily observable, and before the end of three weeks the patient declared that he had never felt better in his life. He returned to Manchester on the 23rd of January.

ART. 115.—*Removal of Laryngeal Polypus after Preliminary Division of the Thyroid Cartilage.*

By Drs. ULRICH and LEWIN.

(*Wien. Med. Wochenschr.*, Feb. 1, 1865; and *British Medical Journal*, March 11, 1865.)

According to Lewin, two cases only of this operation had been previously recorded; one which was performed by Ehrman of Strasburg in 1844, and another said by Pirogoff to have been performed some time ago in Heidelberg. The present case is interesting in its result, as compared with that of Ehrman. In Ehrman's case, the voice was not restored after the removal of the tumour; in that related by Ulrich and Lewin, there was perfect aphonia before the operation, but afterwards the patient was able to speak distinctly, in a somewhat deep bass voice.

CASE.—The patient was a girl, aged sixteen, who four years previously had, without any known cause, suddenly become hoarse, and at last lost her voice. On a laryngoscopic examination being made by Dr. Lewin, polypus growths, especially on the left false vocal cord, were found to be the material cause of the aphonia, and also of considerable impediment to respiration. As a preliminary measure, a canula was, on October 8, introduced through the crico-thyroid ligament, with the effect of rendering the breathing perfectly free. On October 31, Dr. Ulrich, with the assistance of Dr. Lewin and two other colleagues, put the patient under the influence of chloroform; the thyroid cartilage was divided and held apart by blunt hooks, and the canula was removed; the polypus, on which the light of a lamp was thrown by means of a mirror, was seized with hooked forceps, and cut off by curved scissors. The polypus growths, two in number, were seated inside the laryngeal ventricle and on the false vocal cords; their bases were not broad, and it was found necessary to remove them separately. The two tumours formed together a mass of about the size of a hazel-nut. After removal, caustic was applied to the points whence the tumours had arisen; the canula was re-introduced, and the wound made in the operation was brought together with strips of plaster. On the third day the canula was removed, as the patient could breathe freely through the normal passage. On November 23, the wound was nearly cicatrised, and the firmness of the larynx showed that the divided halves of the thyroid cartilage had united. On laryngoscopic examination, Lewin subsequently found that, in speaking, the edges of the false vocal cords became perfectly approximated and almost closed the glottis, while the true vocal cords were red and swollen, moved very imperfectly, and were far from closing the glottis. He attributes the remarkably deep bass voice of this young patient to the abnormal activity of the false vocal cords, which here acted vicariously for the true cords.

ART. 116.—*A Growth springing from the Epiglottis successfully removed with the Aid of the Laryngoscope.*

By Dr. GEORGE DUNCAN GIBB, Assistant-Physician to the Westminster Hospital.

(*Proceedings of the Royal Med.-Chir. Society, Nov. 8, 1864.*)

CASE.—The patient was a lady of sixty years of age, the wife of a clergyman, who had suffered from a throat affection for two years, the symptoms simulating malignant stricture of the œsophagus, with more or less constant dysphagia and expectoration of blood and mucus, associated with severe pain. Matters had been getting worse for the last twelve months. On examination in July last, she was pale and wan, emaciated, spoke in a low, thick, guttural tone, had complete dysphagia with fluids, and could swallow a little farinaceous food or an egg. Fluids always passed through the nostrils. She had no dyspnoea unless when lying on her back at night, but never in the daytime; coughed and expectorated mucus, at times frothy or thick and stringy, and sometimes mixed with blood. With the laryngoscope, a roundish, prominent, and projecting tumour, of the size of a small walnut, was seen occupying the position of a pendant epiglottis, which, during the act of swallowing, became elevated, and could be seen by the unaided eye at the back of the mouth pressing against the pharynx. The epiglottis itself was not seen, nor the interior of the larynx; but the posterior margins of the arytenoid cartilages could be observed. It seemed as if the tumour, partly divided by an antero-posterior sulcus, grew from the lingual surface of the epiglottis. It was red and vascular in some parts, white in others, eroded, and giving rise to free expectoration of mucus and oozing of blood. Believing that the entire cartilage was involved in the disease, the author's intention was to remove the whole mass, including all the free portion of the epiglottis, and trust afterwards to nature to so contract the laryngeal orifice as to permit swallowing without inconvenience, as had happened to him in other cases where the free portion of the cartilage had been destroyed by ulceration. On the 7th of July the thick loop of wire of an improvised écraseur was passed around the base of the tumour, and, on the second introduction, it was drawn home, and quickly detached the growth; and at the same instant, it was seized by Mr. Ure at the back of the mouth with vulsellum forceps, and extracted. Chloroform was not administered, the lady having sat on a chair in front of the author, the tongue being held outwards by Dr. Logan. There was little bleeding, and it was found that the lingual surface of the epiglottis had been separated from the growth as cleanly as if dissected with a scalpel. The subsequent progress was most satisfactory: the character of the voice improved, and swallowing now became quite easy, and has continued so to the present time, although occasionally there is a little sanguineous expectoration from the throat. The tumour was soft and spongy, and, on microscopic examination by Dr. Andrew Clark, was pronounced to be unquestionably benign, in its present condition a connective tissue, and would have become most probably a fibro-cartilaginous growth. In a general commentary upon the case, the author believed that the growth had become movable, with a sulcus running transversely behind, which permitted of its fortunate removal without injury to the epiglottis itself.

(B) CONCERNING THE TRUNK.

ART. 117.—*A Case in which the Innominate, the Carotid, and Vertebral Arteries were tied.*By Dr. SMITH, Surgeon to the Charité Hospital,
New Orleans.

(Lancet, Jan. 14, 1865.)

CASE.—The patient was a mulatto, aged thirty-three, and the tumour large, with strong pulsations. Dr. Rogers, who was requested to be present at the operation, stated that it would not succeed, and advised the deligation of the innominate, and the carotid, as he had proposed in 1849. The procedure was put off for a few days, and on the 15th May, Dr. Smith, in the presence of several civil and military surgeons, tied the innominate and the right carotid at about an inch from their origin. The wound was dressed in the ordinary way, and the patient put to bed. On the 28th, the thread of the carotid fell, and on the 29th, hæmorrhage occurred and was arrested by a slight compression. On the 30th and 31st, hæmorrhage again occurred. On the 1st of June, the bleeding taking place again, Dr. Smith removed the lint, and filled the wound with shot. On the 2nd, the ligature of the innominate became detached. On the 17th, a portion of the shot was removed from the wound, and, as bleeding was observed a few hours afterwards, the shot was replaced. Hæmorrhage was seen again on the 5th and 6th of July. Believing the bleeding to proceed from the vertebral artery, through the subclavian, Dr. Smith tied the vertebral artery on the 9th of July. No hæmorrhage up to the 19th, when the thread applied to the vertebral fell. Some pulsation seemed to be noticed in the radial, and the aneurismal tumour had disappeared. On the 30th, the general health was much improved, and the wound almost closed. The patient was walking about the ward, and expressed a wish to return home. On the 9th of August the tumour had vanished, the wound was healed, and the patient walking about.

ART. 118.—*Case of Ilio-Femoral Aneurism cured by Pressure on the Common Iliac and Superficial Femoral Arteries under the Influence of Chloroform.*By Mr. E. D. MAPOTHER, Surgeon to the St. Vincent's
Hospital, Dublin.

(The Medical Press, March 29, 1865.)

CASE.—John Dunne, aged twenty-five, a small and rather muscular man, a druggist's bookkeeper, was admitted on January 4th, by recommendation of Dr. M'Cormick, who had diagnosed an aneurism involving the right external iliac and common femoral arteries. The patient had been healthy, a primary syphilitic sore and a suppurating bubo in the site of the present tumour, for which he was salivated four years ago, having been the only disease he remembered to have suffered from. Five months before admission he had remarked a swelling about the size of a hazel-nut in the right groin, which gradually and painlessly increased for three months. Its pro-

gress then became so rapid that when admitted it was the size and much the shape of a large hen-egg, extending along Poupart's ligament with three-fourths of its vertical measurements below that line. It was moderately hard, and the sac seemed to come within a uniform distance of the surface. It had a vigorous expansive pulsation synchronous with the heart's beat, and which became more jerking when the artery below the tumour was compressed. A thrill and murmur were also very distinct. He had no permanent annoyance save such stiffness in the hip-joint as would result from so considerable a swelling in its flexure, but he was able to walk pretty well, and the osseous and articular structure did not appear to have suffered. He had occasional darting pains along the crural branch of the genito-crural nerve. The whole limb was oedematous. The tibial arteries were pervious, and as well as all other parts of the circulatory system, appeared to be healthy. The pulse was 88 while in the horizontal posture. Pressure on the external iliac artery controlled the pulsation, but was so painful that the patient could not endure it for a minute at a time. Pressure on the aorta or common iliac artery was still more unendurable; for so full was the abdomen, the bowels having been habitually constive, that it required to be most forcible before the flow of blood was stopped. Spasm and respiratory contraction of the abdominal muscles would also raise off the compressing force now and then. Ice was applied to the tumour; and to lower the pulse, perfect rest, with the trunk horizontal and the limb elevated and supported by pillows, was enjoined, and he was given full doses of digitalis.

With regard to diet, he was allowed as much albuminous food as he was able to consume, and as little fluids as he was content with. From such food it may be expected that really plastic blood will be elaborated; for although low diet will no doubt increase the quantity of fibrin, such will be of an aplastic character ill-calculated to consolidate the sac firmly. Starvation increases the proportion of fibrin in the blood by causing the copious addition of lymph-fibrin, which it should be remembered is not coagulable till further oxygenated. If fibrin of any kind would suffice, fibrin would be our most easy, rapid, and effectual way to increase it; yet we know that, except in very hyperæmic cases, this step is most disadvantageous. With regard to denial of drink, physiologists tell us that in this way we cannot alter the proportion of water in the blood. His bowels were carefully regulated. At the end of a week the oedema had disappeared, the arterial and general strength had diminished, but with these exceptions no change had occurred; yet as the tumour was quiescent, and pressure was too painful to be thought of, we persevered for a month in this palliative treatment.

Feb. 15th.—For two days from this date digital pressure was kept up upon the superficial femoral, just beyond the sac, without any effect, perhaps because the profunda carried off the blood which might be expected to clot.

Feb. 20th.—On examining the tumour with Dr. O'Ferrall, who took the greatest interest in the case throughout, and to whom I was indebted for many practical suggestions, I was distressed to find that it had increased greatly towards the pubis, and as it had suddenly extended along the descending ramus of that bone, it really appeared that the sac had given way in this direction. Many of the leading hospital surgeons of this city saw the case on this day and the following morning, and most of them regarded the ligature of the external iliac artery high up, or that of the common iliac, if the former were found diseased, as imperative. At the suggestion and with the aid of one most judicious surgeon, I gave flexion a short trial, but as it was found so painful, and had no effect on the vigour of the pulsation, I soon discontinued it.

On the following morning (21st) I began pressure, having somewhat

blunted the patient's extreme sensitiveness by twenty-drop doses of Battley's sedative every third hour. The tumour having extended more than an inch above Poupart's ligament, pressure on the external iliac was unadvisable, and, indeed, nearly impossible. I therefore determined to bear on the common iliac; and the skin of the region having been shaved, Carte's admirable compressor was carefully fitted, but when the pad was screwed down tight enough to moderate the pulsation, the pain beneath it, in the tumour, and along many of the cutaneous nerves, was so excruciating that he could not bear it for two minutes. A seven-pound leaden weight (cast into various shapes) being equally unsuccessful, I had recourse to digital pressure, which the patient consented to bear, though it pained him considerably. At first the thumb was pressed down upon the right common iliac as it passes over the fifth lumbar vertebra; but as that member tired in three or four minutes, the force required being very great, and as the blood passed freely through the artery each time it was being relieved, no matter how expertly, a large cork was cut so as to present a convex oval surface across the vessel and a concavity into which the thumb fitted. The skin was protected by a double fold of damp chamois. By this pressure all pulsation could be stopped for from ten to fifteen minutes, when it recurred as the thumbs were being exchanged. In this way, and with such unavoidable intermissions, pressure was maintained for six days and nights by the pupils of the hospital, the patient sleeping about four out of the twenty-four hours. I feel it just to say that the zeal and never-flagging patience of these gentlemen were indeed praiseworthy, and proved that they were morally qualified to follow the benevolent calling to which they aspire. Among them I must particularly mention Messrs. E. Nugent and Adye Curran, and Mr. Booth, until disabled by a dissection wound. To the former I am indebted for many of the notes of the case. During the whole period, in addition to the opium, large doses of tannic acid and bromide of potassium were administered, and to the influence of the latter drug we attributed a considerable fall in the frequency of the pulse. On the seventh day the aneurism was perhaps a little harder round the base, but in the centre it was soft and so near the skin as to be truly formidable, and I determined to tie the common iliac or external iliac very high up, selecting, from anatomical considerations, the lumbar method of Sir P. Crampton.

By that night's post I received from my friend Dr. Ellis, of Newcastle-on-Tyne, an account of a case of aneurism of the abdominal aorta cured by pressure on that vessel, the patient having been kept all the time under the influence of chloroform. The case occurred in the practice of Dr. Murray, Lecturer on Physiology in the University of Durham. "The pressure and insensibility were kept up for about five hours; until the last hour the slightest movement in the tourniquet showed that pulsation in the tumour had not ceased, and that the disease was unaltered. As an instance of the dependence of a curative process on the influence of chloroform, this case is most striking, for no man exhausted with pain and weary of life could have borne for five hours without an anæsthetic such tremendous pressure as was here employed, even though that pressure were to save his life." Five months after the tumour had disappeared, one femoral was pervious, the other occluded.

Encouraged by this case, I determined to postpone deligation, and employ compression with the aid of chloroform. Next morning (Monday 27th), at eight o'clock, I chloroformed the patient, and at once stopped all pulsation by pressure with Carte's apparatus in the common iliac artery about one inch below the umbilicus, and half an inch to its right side. The skin was shaved, and protected by finely-powdered French chalk. The stoppage in

the circulation was never complete for more than a few minutes, for the movements of the patient, the action of the abdominal muscles, and the rolling of the vessel under the pad, allowed an occasional slight current to pass through it. Severe vomiting occurred at seven P.M., and rigors succeeded every fifteen or twenty minutes, and for these reasons the administration of chloroform was discontinued at eight P.M., having been maintained for twelve hours, and the patient at once feeling pressure intolerable, the instrument was removed. I was greatly disappointed to find the tumour unimproved, or indeed more flaccid and soft than before. He slept a little during the night, but suffered occasionally from sickness of stomach.

The next day there was a red patch with six or eight vesicles where the pad had pressed, over which the patient complained of extreme tenderness. He was very prostrate and no longer hopeful of our efforts. For the next few days, any operation being evidently out of the question, we relied on careful nursing and gentle stimulants, under which his constitutional symptoms improved, the tumour remaining unchanged.

On Thursday I heard again from Dr. Ellis that Mr. Heath, of the Newcastle Infirmary, had successfully compressed the aorta under chloroform for an aneurism of the external iliac. The pressure and anæsthesia were maintained for seven hours, for the last half hour of which all pulsation in the sac had ceased.

On Saturday, March 4th, as my patient had considerably recovered his strength, I resolved to try pressure with the help of chloroform once more, and as the red and exquisitely tender patch produced by the former attempt reached the umbilicus, the aorta seemed the only available vessel.

The following preparatory steps, to which I attach much importance, were taken :—A purgative having been given the night before, I threw up a pint of tepid water into the rectum, which, coming away with some feces and much flatus, left the abdomen very lank. If any tympany remained I intended to have left a large tube in the rectum to allow any gas to escape as soon as formed. He also emptied his bladder. As pressure on the vena cava or right common iliac vein could be scarcely avoided, I aided the venous return by a tightly applied flannel roller, and the sac was compressed by an elastic bandage put on spica-wise round the groin and hip. The limb was elevated, and to render any increase of blood for muscular action unnecessary, it was firmly fixed.

The patient having been rendered quite anæsthetic, the superficial femoral, according to Dr. O'Ferrall's suggestion, was compressed by Skey's tourniquet just below the sac, in which most of the blood was in this way retained. The horse-shoe clamp, Signorini's simple and effective instrument, consisting of two arms, the one carrying the compressing pad—about two inches by one and a half—the other bearing a hollow pad about six inches by four, just filling the loins, and both connected by a screw and rack, was placed one inch above the umbilicus, and therefore bore on the aorta just above its bifurcation, where it is immediately invested by peritoneum. When the screw at the junction of the arms was turned to its utmost all pulsation in the tumour and in the opposite limb stopped. From nine A.M. till half-past eleven the pulsation occasionally recurred by the patient moving, which shifted the clamp towards the right side. At that hour, with the assistance of Mr. Collis, who had kindly lent me the clamp, it was tied over to the left side by a bandage wound round both its pads. When thus adjusted it was found that the left femoral pulsated, indicating that pressure was now on the right common iliac. However, as the pad was one inch above the umbilicus, the aorta must have bifurcated one and a half inches higher up than usual. For four and a half hours uninterrupted pressure was maintained; and on removing

the instrument at the end of that period, we were indeed overjoyed to find the tumour solid and entirely pulseless. He had two severe rigors, which we relieved by brandy and external warmth. The external iliac was pervious to the sac, but no pulsating vessel could be discovered in the limb. He slept fairly during the night. The following morning he was distressed by severe neuralgic pains and tenderness in the skin where the long saphenous and musculo-cutaneous nerves are distributed, and which we relieved by applying ice. The tumour was extremely hard, save for the size of a hazel-nut, which was soft and fluctuating, probably from the presence of serum, for on the fifth day these characters had disappeared. His strength and appetite gradually improved, and the size of the tumour daily diminished, at first with great rapidity, while the serum was passing away.

At the point where pressure had been kept up for twelve hours the areolar tissue had been for long enough deprived of blood to kill it, for there was formed a true anthrax. By strapping round the base much sloughy areolar tissue was forced out, and it did not extend. To-day (March 24) he sat up for an hour; the tumour is quite hard and rather larger than a walnut. Pulsation cannot be felt in any vessel; yet so efficient are the small anastomosing arteries and the capillaries, that the limb is as warm and as sensitive as the other.

ART. 119.—*On the Relative Advantages of Puncturing the Bladder by the Rectum, and of Perineal Section for the Treatment of Impervious Stricture.*

By Mr. GEORGE POLLOCK, Surgeon to St. George's Hospital.

(*Medical Times and Gazette*, December 3, 1865.)

Without going into the history of the various operations for impervious stricture, Mr. Pollock confines himself to a comparison of the two mentioned in the title of the paper. Neither operation ought to be had recourse to except in those cases in which the catheter cannot be passed, in which the patient cannot evacuate the bladder, in which the urine escapes in dribblets, or incontinently and continuously, and in which the patient has recurring attacks of retention, relieved by constant recurrence to opium or the hot bath, and in which disease of the urinary passages and kidneys is threatening or developed. Now, when the ordinary means of treatment fail—and they should always be first thoroughly tried—we have for the impervious stricture three alternatives. “The first I allude to only to condemn it: I mean the forcible passage of the catheter into the bladder.” It is always confusing, uncertain in effect, and no catheter should be used for this purpose. Many disasters also may, as post mortems reveal, attend this operation. The other alternatives are perineal section and puncturing of the bladder. The first of these requires much care and correct anatomical knowledge; and as the membranous portion of the canal is usually in retention detected behind the stricture, it is preferable to cut into it and pass a catheter into the bladder, and then to unite the posterior pervious portion with the anterior healthy portion by cutting through the stricture. Among the difficulties of this operation the urethra may be missed in cutting down to it. This has been known to happen

to the most experienced surgeons. In this operation there is always some, and often considerable hæmorrhage; at times it is even excessive. Another possible evil is, that the wound may never heal, but become fistulous, although a free proper passage through the urethra may be restored. The author differs from Mr. Cock about the importance of the tracing out of the stricture; even at the deadhouse it is at times very difficult to do so; and there is no objection to a part of the urethra being formed out of new tissue. For one set of cases only does the author consider perineal section preferable to puncturing the bladder—namely, for inveterate and unyielding strictures in young men, the result of laceration of the urethra from injury, when extravasation of urine takes place, and often a large portion of the urethra is destroyed by slough. There seems no alternative in such cases. There is, moreover, generally a healthy state of urinary organs, and the constitution of the patient is not generally impaired. In these cases the stricture is the result of destroyed tissue from laceration, and no cases are more difficult to relieve by the use of catheters without division of the stricture. In these cases the retention is not from spasm, but the result of the slow, unyielding contracted cicatrix. But for the greater number of cases of impervious stricture, the author believes puncture of the bladder to be the preferable operation; and as being the most simple and effectual, the author recommends puncture by the rectum. In his own hands, the operation has been most satisfactory, both in stricture threatening to induce kidney disease and in retention. The catheter should be fastened in with great care, and kept in some time, the patient remaining on his back. Nor is it injurious if kept in for some weeks; and for some weeks also it is as well to keep the urethra quiet; and at the end of that time the surgeon will rarely fail to pass a catheter through it into the bladder. It is then wise to keep it there for a week or ten days; and after that occasionally, according to the tendency to return. When we compare the difficulties and dangers of the one operation with the simplicity and safety of the other (puncture by the rectum), it would not be a bad rule for practitioners to adopt the latter in every case of retention dependent on stricture, rather than that an inexperienced operator should have recourse to perineal section. When it is considered that the one operation is always difficult, sometimes unsuccessful, always attended by bleeding, often by excessive hæmorrhage, always requires the aid of assistants, and must be performed in a good light; is followed by much discharge, and often abscess or pyæmia, and that the stricture, when thoroughly relieved, has usually a tendency to recur; when, on the other hand, puncture of the bladder through the rectum is simple, expeditious, relieves thoroughly, occasions no loss of blood, no local mischief, and equally with perineal section relieves the stricture, and that it is adapted to all cases except laceration of the urethra and its consequent strictures—we may fairly conclude that as a general principle for the relief of impervious stricture or for retention of urine in such cases, puncture of the bladder is the rule, and perineal section the exception.

ART. 120.—*On Puncture of the Bladder.*

By M. VELPEAU.

(British Medical Journal, Nov. 19, 1864.)

In some clinical observations on this subject, delivered not long ago, M. Velpeau says:—

“Puncture of the bladder is a rare operation, which surgeons, even in large practice, do not practise more than once or twice in their lives. Puncture by the rectum is bad, because, in performing it, a fold of peritoneum is traversed. The hypogastric puncture is, also, always to be dreaded; there are many tissues punctured; and, as the bladder is emptied, the canula is liable to fall or slip out of it, and so allow the escape of urine into the peritoneum. Even with an elastic catheter there is no security. M. Roux was much blamed for practising forcible catheterism; but in much too general a way. Retention of urine is due either to stricture of the urethra or to prostatic disease. Forced catheterism is bad in the former case; but not in the second. If the prostate be torn, no great inconvenience results. The prostate is formed of a tissue which does not readily admit of infiltration. M. Ségalas remarked that, of three surgeons who reported on the case, two of them had only performed puncture of the bladder once, and the reporter himself never. M. Velpeau does not wish it to be understood that puncture is never necessary. Success in catheterism depends much on the skill of the operator. The urethra has been compared to an undistended wet linen tube. One surgeon, ten surgeons may have failed, then comes the eleventh, who succeeds.”

ART. 121.—*Congenital Hydronephrosis in a Boy Four Years Old repeatedly and successfully tapped.*

By Dr. HILLIER, Assistant-Physician to the Hospital for Sick Children, &c.

(Proceedings of the Roy. Med.-Chir. Society, March 14, 1865.)

CASE.—The patient was born with great enlargement of the abdomen, simulating ascites, for which it was mistaken till he was nearly four years old. It was then ascertained to be an enormous cyst springing from the right lumbar region. From its great size it caused difficulty of breathing and prevented his walking. The cyst was tapped in front and 102 fluid ounces of clear non-albuminous fluid was drawn off, having all the characters of dilute urine. The fluid rapidly re-collected, and on a second tapping was found to be albuminous and purulent, but still to contain a considerable quantity of urea. Attempts were made to establish a permanent fistula anteriorly, and then posteriorly; but on each occasion the fluid after a time ceased to flow. Much irritation and depression followed the several tapplings, so that the patient's life seemed to be endangered. After one of the operations a quantity of fluid was passed from the bladder exactly similar to that from

the cyst, and quite unlike what was usually passed from the urethra; a temporary communication thus obviously being established between the cyst and the bladder. The patient has now been left without operation for some months, and has regained his strength; but the cyst remains varying from time to time in size, and his urine is often purulent and fetid. It is presumed that there is some congenital malformation of the right ureter which renders it liable to occlusion, but admits, under some circumstances, of the passage of fluid.

Cases of congenital hydronephrosis due to obliteration of the ureter were quoted, proving fatal in infancy; one a case of an enormous cyst, apparently a dilated kidney, from obliterated ureter, in a woman who lived to the age of twenty-three years; and one of double hydronephrosis in a youth who lived to the age of seventeen years. In the latter case the ureter on one side was much constricted, and on the other entered the pelvis of the kidney obliquely, and was guarded by a valvular obstruction.

Expectant treatment, the author observed, seems to be the only measure indicated. Extirpation of the cyst is inadmissible from the dangers of the operation, owing to proximity to the sympathetic ganglia, and to the liability to hæmorrhage and peritonitis. Tapping was recommended in case of distension so great as to endanger life. It would seem that when the distension reaches a certain point, the ureter allows of fluid to pass down it.

ART. 122.—*A Peculiar Case of Lithotomy.*

By Dr. KELBOURNE KING, Hull.

(*British Medical Journal*, Nov. 19, 1864.)

CASE.—On October 26th, 1863, I was summoned by Mr. Burnham, surgeon of Preston, in this neighbourhood, to go to that village, and to take such instruments with me as I would require for the operation of lithotomy. Dr. Sandwith of Hull accompanied me. On arrival, we found our patient to be a farm labourer, about seventy years of age; and we received the following account of his history.

Thirty years ago he had been operated on for stone in the bladder, by the late Mr. Fielding of Hull; and shortly afterwards found his symptoms return. Since the time of the operation, he had never been able to pass urine through the urethra, but evacuated the bladder through the fistula which remained in the perinæum; and of late, since his symptoms became aggravated, he thought that a great part of the urine passed through the anus. He was in an extremely emaciated condition; was in great and constant pain; and was so reduced by suffering that he was glad to submit to anything that might be proposed for his relief.

On examination, the urethra was found to be occluded, and perfectly impermeable to any instrument which I had with me. There were several sinuses in the perinæum; and, on passing the finger into the rectum, I found that a fistulous opening existed about two and a half inches from the anus, opening into the bladder, and permitting the finger to touch a calculus. From this examination, it appeared that the calculus was of very great size, as it could be felt in all directions, filling up the space usually occupied by the bladder.

As no staff could be passed, and the man was reduced to a state of the greatest misery and depression, the following operation was performed, after consultation with Dr. Sandwith and Mr. Burnham. I passed the index

finger of my left hand into the bowel, and placed its point upon the stone; then introduced a strong, straight, probe-pointed knife through the anus, and passed it up to the finger already introduced, turning the sharp edge forwards. An incision was then made through all the soft tissues—bowel, sphincter, and perineum—strictly in the middle line, from the sinus in the rectum to about an inch in front of the anus. Free scope being thus afforded, the forceps were introduced; and it was found that the stone occupied the whole interior of the bladder, which seemed to be moulded, as it were, upon the calculus. By the use of scoop and forceps, the walls of the bladder were lifted off the stone, which was then, after some difficulty, shaken out of its position; and finally, though not without some expenditure of time and patience, was removed. The last stage of extraction was facilitated by the fracture of the calculus into two pieces.

The old man was a good deal exhausted at the close of the operation; but when I saw him half an hour afterwards, he was smoking his pipe, and declared himself to be feeling tolerably well.

I have not seen him since; but I believe he recovered without a bad symptom; and I heard of him as following his usual out-door occupations some weeks after.

The stone, which was globular in form, weighed 6 ozs. 2 drs., and measured $7\frac{1}{4}$ by 8 inches in circumference; and there was a great deal of débris besides. It was exhibited to the meeting.

ART. 123.—The “*Modus Operandi*” in Successful Lithotrixy.

By Mr. HENRY THOMPSON, Surgeon Extraordinary to H.M.
the King of the Belgians, Surgeon to University
College Hospital.

(*Lancet*, March 4, 1865.)

While there appear to be some leading general principles which direct the practice of lithotrixy, there is no operation in surgery, perhaps, which depends more on attention to minute details for its successful performance.

The general principles may be thus stated:—

1. That mechanical contact with the bladder and urethra, whether from instruments or from rough fragments of calculus, is to be reduced to the smallest possible extent.

2. That the natural functions of the urinary organs are to be as much as possible rendered auxiliary to the operation; and that they are to be interfered with, and, *à fortiori*, to be impaired as little as possible.

“In illustration of the first principle—viz., to avoid all unnecessary instrumental contact with the bladder and urethra, I may say that, in the nineteen cases related in last week’s *Lancet* as having been operated on by me during the year 1864, and comprising 124 distinct sittings, not one preliminary injection was employed, nor was there a single washing out afterwards to remove débris. The lithotrite was rarely introduced more than once at a sitting, never more than twice, and no sitting exceeded five minutes in duration; the majority did not last three minutes. Lastly, a large fragment

was never withdrawn by the lithotrite. By this practice, a great amount of mechanical interference was spared to the patient. I think I have seen as much harm result from rapid and forcible distension of the bladder by injections for the removal of débris as from any other part of the operation of lithotrity, excepting always, it may be said, unnecessarily rough and frequent use of the lithotrite. The evils which follow rapid distension of the bladder in washing out forcibly are, chronic cystitis and atony of its coats. No doubt the former condition is often treated in the most efficient manner by washing out gently, and with small quantities of fluid slowly thrown in; but that is a kind of washing which is useless to remove débris.

"On the same principle, I greatly prefer to avoid any unnecessary introduction of the lithotrite. The object has been to use an instrument which will pulverize the stone as much as possible, so that it may be ejected during the natural efforts of the bladder, and not to remove more débris in the blades of the lithotrite than can be easily withdrawn without encountering resistance, so as not to damage the neck of the bladder or the urethra.

"The sitting, also, should be short—two minutes for the presence of the lithotrite in the bladder, suffice for all ordinary purposes; sometimes three minutes are necessary. Ample time is thus afforded to seize and crush fragments, four, five, or six times, employing the flat-bladed lithotrite with no opening in the female blade (so often misnamed or confounded with the 'scoop,' a wholly different instrument), and removing with it only as much as can be contained without causing difficulty in the withdrawal. Rarely should there be more than a slight appearance of blood, either at the time or in the first urine subsequently passed; often there will be none. The operator requires no other instrument with him than the lithotrite, and for so short and unirritating a proceeding he requires no chloroform and no preparation. The urine may have been voided half an hour or an hour previously; it is of small consequence, provided that too much is not present, a large quantity usually making it less easy to seize the stone. The patient soon loses his dread of a proceeding so little disturbing; and, as the sitting is rarely followed by a chill, it may be repeated every fourth or fifth, sometimes, in exceptional cases, every second or third day, without inconvenience. If more than ordinary soreness or irritability of the bladder is found on the introduction of the lithotrite, it is withdrawn, or there is a shorter sitting than usual, and so a chill may be avoided; conditions which, under anæsthesia, would not be recognised. Thus, it may be added, that in two only of the patients referred to (the single child's case of course excepted) was chloroform administered, and each of those at the first sitting only; the second was submitted to, at my request, without it, after which each patient preferred to dispense with chloroform, and had all the subsequent sittings without it. (Cases 1 and 12.)

"The flat-bladed lithotrite, referred to above, has the singular merit of finely pulverizing the large fragments very easily and expeditiously; for the object is not only to avoid irritating the

urethra, and especially the neck of the bladder, by withdrawing large calculous fragments with rough and sharp angles, but, in conformity to the first general principle, even to hinder such fragments from traversing the canal by themselves. When thus pulverized, the results of the sitting are easily passed by the performance of the natural functions, which, after the operation has been completed, even the elderly patient ought almost always to retain unimpaired. But when large fragments have been drawn through the neck of the bladder, retention of urine often follows, not to say cystitis, and the expelling function is often temporarily, sometimes permanently impaired, or even destroyed. After the first or second crushing of a full-sized stone, when it is necessary to use a powerful and fenestrated instrument, large fragments must necessarily result, and very little fine débris indeed is made; but, so far from desiring to remove these, I cannot but regard it as an important maxim to discourage them passing at first. By keeping the patient in bed for twenty-four or thirty-six hours after the first sitting, desiring him to pass urine only in the recumbent posture, and if he can, while lying on his back, irritation is reduced to a minimum; the fragments do not pass, but soon get slightly waterworn, their sharpest angles rounded, after which, if expelled, they pass much more easily than at first. The urethra itself has had time to recover from any soreness or irritation which it had acquired. Both the canal and the fragment are better adapted to each other than at first. I would rather see only a little débris pass until after the flat-bladed lithotrite has been used to pulverize the fragments, when the product will be expelled easily enough. On these principles, the rule is to interfere with nature as little as possible. Nineteen-twentieths of all calculi are expelled by her without any help from us; for those of the remainder which come under the hands of the surgeon, she will aid us most efficiently and most safely if we will be content to trust her, and will not interfere to do roughly what she can do well and easily for herself. Time, a little more time, may be necessary to the process; but that is a small price to pay for increased safety to the patient. In this, as in other surgical proceedings, the *nimis diligentie* appears to be the especial source of numerous evils.

"It would be affectation on my part to ignore the fact that the practice of removing fragments as little as possible by instrument is diametrically opposed to that of a well-known authority deservedly great on the subject of operations for stone; and, further, that the system which Mr. Fergusson advocates of withdrawing many times, during a single sitting, large fragments from the bladder, is even regarded by some as an improvement in lithotripsy. With great deference to one whose experience and opinions I, in common with my brethren, so highly respect, and from whom, indeed, I regret not a little to differ so widely, I feel compelled to protest against this view of that practice. I have no alternative but to say that increasing experience confirms me in my adherence to the principles laid down as the very foundation of successful lithotripsy—viz., to diminish mechanical contact with the bladder

and urethra as much as possible; and to crush so efficiently that the débris can come away easily by itself. At all events here is the fact, that, by a rigid adherence to these principles, nineteen consecutive cases of lithotritry, the majority of patients of very advanced age, and some with large stones, were brought to a successful termination during the past year without that amount of instrumental interference which is very commonly employed. Further, in not one of these cases has the function of passing water been impaired by the proceedings adopted."

ART. 124.—*The Peculiar Applicability of Lithotritry to Cases of so-called "Paralysed Bladder."*

By Mr. HENRY THOMPSON, Surgeon Extraordinary to H.M.
the King of the Belgians, Surgeon to University
College Hospital.

(*Lancet*, March 4, 1865.)

There are several morbid or unusual conditions of the urinary organs, apart from the size and texture of the stone, which it was formerly the custom to regard as contra-indicating the performance of lithotritry, all of which it is certainly not now necessary to consider in that light. Among them is that affection in which the patient has lost the power of passing any urine whatever by the natural efforts, and is compelled to remove it several times daily by means of the catheter. This condition has been usually termed, although not very happily or accurately, "paralysis of the bladder." For in most instances the loss of power to pass the urine naturally is due, not to any impairment whatever of the nervous supply to the organ, but entirely to obstructive disease of the urethra, such as prostatic enlargement, impeding the action of an otherwise healthy bladder. In some cases, again, it may be due to loss of contractile power in the muscular coat of the bladder, produced by some accidental, perhaps voluntary, over distension—in other words, to atony of that viscus; no obstruction existing in the course of the urethra. And, lastly, these conditions may co-exist to a greater or less extent in any given case.

"I have for some time been led to regard such a condition of the bladder not merely as one in which lithotritry ought not to be performed, or might be performed with difficulty; but, on the contrary, as one which is peculiarly favourable to the success of the operation. I have no hesitation in regarding it as presenting circumstances more likely to conduce to a happy result than those which are presented by the ordinary or average run of calculous patients in middle life who have perfect command of the urinary functions. Nevertheless, examples of success in the cases referred to are still sometimes spoken of as unusual or exceptional instances. I ventured to express the opinion given in the Lettsomian lectures of 1862, and I am no less convinced of its correctness now. The

reason seems to be this, that in cases of patients who have been compelled to use the catheter several times daily for some months or years, the passages have become so accustomed to instruments that the pulverized débris may be withdrawn by the lithotrite with comparative impunity; and in these I do not hesitate to do so, still avoiding the removal of fragments which would irritate the urethra and occasion bleeding. I have operated on several such cases, and have had none that gave less trouble or made better recoveries. It is rare for such patients to suffer from rigor or other constitutional disturbance, for the passages are callous to those sources of irritation which in the patient of ordinary susceptibilities, and unaccustomed to the employment of any instrument in the urethra or bladder, so often arouse systematic derangements of a serious nature.

"I believe that it is an error to regard the small calculus recently descended from the kidney into the bladder of an apparently healthy middle-aged man as a matter of very slight gravity and of small surgical importance. I have occasionally seen more distressing and prolonged illness arise from the crushing of such a stone—although only one short and easy sitting sufficed to readily accomplish the object—than from stones of ten times the size. The very presence of a stone in the bladder for a few months, irritating as it is, seems sometimes to render the bladder accustomed to mechanical contact; and the lithotrite may sometimes be used more safely in such circumstances than in a bladder which has but recently become tenanted by a stone, or which has never been entered by a catheter or sound. The cases of small and recent calculus are those which, perhaps as much as any, benefit prospectively by passing bougies a few times preparatory to the subsequent use of the lithotrite. With all the usual precautions the case of small calculus becomes one of extremely favourable augury; but it is hazardous to be in the least degree indifferent to precautions because the calculus is small or recent.

"I have operated in seven cases in which the patients had for years depended on the catheter to evacuate the bladder: in one instance from real spinal paralysis; in the other six, from atony of the bladder or from enlarged prostate. One only ever had rigors; all made excellent recoveries."

ART. 125.—*Respecting the Instruments employed in Lithotrixy.*

By Mr. HENRY THOMPSON, Surgeon Extraordinary to H.M.
the King of the Belgians, Surgeon to University
College Hospital.

(*Lancet*, March 11, 1865.)

In order to reduce a calculus to the proper condition for passing from the bladder easily, Mr. Thompson considers two distinct varieties of instruments necessary. He says:—

"The first, a powerful instrument, of which the female blade has a large opening, sufficiently large to admit the male blade, and

through which opening fragments are forced by the action of that blade. The second, a less powerful instrument, of which the female blade is wide, flat, much larger than the male blade, and without any opening to admit the latter.

"The first, or fenestrated instrument, is necessary to the first crushing of any stone at all above medium size, and often to the second and third crushings if the stone is a large one; an instrument of the second class, with flat and non-fenestrated female blade, not being sufficiently powerful for the task. Now the fenestrated instrument requires considerable care in its employment; the edges of the male and female blade are sharp, and fit accurately to each other in order to ensure an irresistible "bite" on the large hard stone. The result of its action on such a stone is the production of large, angular, wedge-like pieces, especially if the stone be composed of uric acid. Much space is also required for its employment; the blades must be opened widely in order to seize a large stone, and, from their own thickness, they project considerably beyond it; they must also have a length proportioned to the size of the stone. Being altogether a large instrument,—a condition necessary to the performance of its function,—it is not very readily moved in the bladder. Admirably adapted to the object—viz., that of reducing the entire calculus to several large fragments, it may be most advantageously replaced by the flat-bladed instrument when that object has been effected. To use it for reducing large fragments to smaller ones, is to employ an instrument which is not well adapted to that purpose, and one which is at the same time liable to produce an unnecessary amount of irritation.

"For the purpose of reducing large fragments to smaller ones, or to débris, the flat-bladed instrument is greatly superior to the preceding. It is a lighter instrument, and occupies less space; it can be opened to the requisite extent without impinging, so much as the fenestrated instrument, upon the walls of the bladder, especially at its neck. Its blades are shorter, it can easily be turned right or left, or rotated completely if need be. The edges of the blades are rounded, since nothing could be gained by making them sharp; they do not even meet each other, an interval always remaining between them when the instrument is tightly closed, so that a tyro need scarcely fear to lay hold of the mucous membrane. Its blades being wide and flat, not hollowed or scoop-like, and without an opening (except a very small one at the angle), the fragments are easily seized, do not readily slip, and when crushed are reduced mainly to powder between two flat opposing surfaces. For the same reason, if the urethra is capacious, the instrument may be withdrawn retaining a good deal of this powder (not sharp fragments) impacted between the blades; while if the urethra is not capacious, it is easy to disengage the greater part of this powder from the blades before withdrawing. As a rule, such an instrument may be used, I venture to say, with advantage, certainly seven times as often as the fenestrated instrument. In the nineteen cases I have reported, with 124 sittings, certainly not less than 100 to 110 were conducted with the flat-bladed lithotrite; indeed, the other was only

used for the larger stones. But the flat-bladed instrument may be constructed on a very powerful model. If the male blade is made about half the width of the female blade, and with a very slightly wedge-shaped contour, very slight indeed, as I have suggested and employed, it will readily penetrate any uric-acid stone of an inch or an inch and a quarter in diameter. It should here be said that if the male blade has much of the wedge-shaped form, like a Δ , for example, it not only cuts quickly into the calculus presented to it, but throws off the fragments laterally with extreme force. I tried the experiment some years ago, and was struck with the dangerous facility with which this happened: such fragments were thrown several feet from the instrument when air was the surrounding medium, and very far in water—a totally different result to that from fracture by the ordinary square male blade. The very slight wedge form above described has also the advantage of obviating impaction of the blades with *débris*, which is more likely to occur with a flat-bladed instrument in proportion as its size is augmented.

“Nothing can be more distinct than the characters of the true flat-bladed lithotrite and those of the instrument which has long been termed a ‘scoop.’ The latter was introduced into practice long before the flat-bladed lithotrite was made. In the scoop the female blade is much hollowed out, as the name implies, and the edges of both blades fit accurately one to the other: for this reason there are strong objections to the instrument; and, further, because its hollowed blade is necessarily easily impacted by *débris*; because a small quantity of *débris* largely augments the calibre of the blades; and, lastly, because this *débris* cannot be dislodged after impaction has taken place.

“Next, in order to reduce mechanical contact with the bladder to its minimum, it is essential to employ a lithotrite of which the two motions, those of the screw and the sliding movement of the male blade, are easily exchanged the one for the other. The lithotrite which has long been used in this country—always by Sir B. Brodie—is one in which the blades cannot be reopened after having been screwed home, except by the tedious act of unscrewing. Professor Fergusson overcame this great defect by substituting the rack and pinion for the screw as the mechanical power. Speaking generally, however, the screw is still much preferred to the rack and pinion, now that its action can be instantly detached, and its blades opened and closed by a sliding action convertible into screw action by a simple movement of the thumb. Regarding carefully all the points of importance, I do not know any instrument, British or foreign, which at this moment so completely supplies all the desiderata as a form of lithotrite which has been recently made by Messrs. Weiss: that is, one which does its work with so small an amount of concussion in the bladder. It can be turned smoothly round with the greatest facility, merely by the finger and thumb of the left hand, in prosecuting the most delicate search; it can be held immovable with the firmest grasp by its cylindrical handle when dealing with a large stone. I have long felt the want of such a handle in the French

lithotrite, which, up to three years ago, appeared to many to be the most commodious instrument, and in consequence suggested the long cylindrical one; and it has answered beyond expectation. The mode in which the sliding action is converted into screw action, and *vice versa*, which Messrs. Weiss have designed and applied, is much simpler, easier to accomplish, and less liable to cause lateral movement of any portion of the instrument within the bladder, than is the French instrument.

"So much for lithotrites. With regard to washing out the bladder for the removal of *débris*, I am sure all who have much employed it must confess how little productive is the process. When much *débris* is within the bladder, no doubt it is easy enough to remove some of it thus: but then no difficulty exists respecting this *débris*. It will pass off by itself readily enough. It is the obstinate fragment at last, or the two or three fragments, that do not come away, but remain in spite of repeated washings, which sometimes demand interference for their removal, and may occasion a little trouble. Some accidental shape perhaps causes this: the fragment may be a flat scale perhaps, which adheres to the bladder, and is not easily transmitted by the natural current, or washed through the eye of a catheter. Whatever the cause, however, so it sometimes happens. For myself, I have preferred in such a case to detect its exact situation in the bladder by means of a small sound delicately used, and, having done so, the patient not moving, then to pass a small, flat-bladed lithotrite straight to the spot, when three times out of four it may be picked up at once, usually with the blades pointing downwards, and found often immediately behind the neck of the bladder.

"I have recently seen, and two or three times used by way of experiment, an instrument for removing *débris* from the bladder, designed by Mr. Clover, which promises to be useful. He has contrived a very ingenious method of making a current of not more than one or two ounces of water pass to and fro from the bladder into a glass receiver, into which the *débris* is deposited. Mr. Clover will, no doubt, describe its action before long. It seems to me much more efficient than the recent application of the ancient method of irrigation by means of the screw, which has been employed by M. Maisonneuve, and one of which instruments I have."

ART. 126.—*On the Solvent Treatment of Urinary Calculi: an Experimental and Clinical Inquiry.*

By WM. ROBERTS, M.D., Physician to the Manchester Royal Infirmary.

(*Proceedings of the Royal Med.-Chir. Society*, April 8, 1865.)

This paper is divided into two parts. The first part is devoted to experiments and observations relating to the solvent treatment of uric-acid calculi by alkalizing the urine by internal medicines. The inquiry starts from two known data—namely: first, that uric

acid is dissolved by solutions of the alkaline carbonates of a certain strength; and, secondly, that alkaline carbonates can be introduced into the urine, so as to render it alkaline, by the administration of certain salts by the mouth. The practicability of dissolving renal and vesical calculi, composed of uric acid, by alkalizing the urine, is inquired into under ten headings or sections as follows:—

Section 1. Comparison of solutions of carbonate of potash and carbonate of soda: in which it is shown that solutions of carbonate of potash are better solvents for uric acid than solutions of carbonate of soda.

Section 2. Comparison of solutions of different strength: in which it is shown that the greatest solvent power (for uric acid) lies in solutions containing from forty to sixty grains of carbonate to the imperial pint. Above this strength dissolution is soon prevented by the formation of a crust of biurate which invests the stone. Below this strength the solvent power gradually declines.

Section 3. Comparison of effects of varying volumes of solutions of constant strength. — It is shown that the quantity of the solution permitted to pass over the stone, between the limits necessarily imposed by the capacity of the kidneys to separate aqueous fluid, is of slight importance. A flow of three or six pints during twenty-four hours was found nearly as effective as a flow of eight or fifteen pints.

Section 4. Absolute rate of dissolution of uric-acid calculi in solutions of the alkaline carbonates.—It is shown that solutions of carbonate of potash, of the maximum solvent power, when passed at the rate of from three to eight pints in the twenty-four hours over uric calculi, at the temperature of the body, dissolve from ten to twenty per cent. of the weight of the stone each day.

Section 5. The most convenient way of alkalizing the urine, the degree of alkalescence which can be communicated to it, and the doses required to produce the desired effect.—The bi-carbonate, acetate, and citrate of potash are found the most effective substances to alkalize the urine. Of the three the citrate is preferred. It is found that forty grains of citrate of potash dissolved in five ounces of water, taken every two hours, alkalizes the urine to a mean degree corresponding with the maximum solvent power of solutions of carbonate of potash.

Section 6. The effect of alkalized urine on uric-acid calculi.—The urine of a person taking full doses of citrate of potash, as recommended in Section 5, is passed over a uric-acid calculus at blood-heat. The stone (weighing 180 grains) loses weight at the rate of twelve grains and a half in the twenty-four hours. In the performance of experiments on this point it came out that if the urine became ammoniacal (from decomposition of urea), it ceased to dissolve the uric acid, and the stone became invested with a crust of precipitated phosphates. Whence the important deduction is drawn, that ammoniacal decomposition of the urine in cases of vesical calculi puts an absolute bar to the effectiveness of the solvent treatment by alkaline carbonates.

Section 7. Illustrations of the application of the solvent treat-

ment in practice ; first in renal calculi, secondly, in vesical calculi.—Two cases of complete dissolution of uric-acid calculi in the bladder are quoted from other authors. The author relates three cases which occurred in his own practice. In none of the latter did complete dissolution occur. One of the cases proved to be an example of mulberry calculus ; another, an alternating calculus of uric acid and oxalate of lime. This second specimen offers peculiarities of surface which indicate with certainty that dissolution of the uric acid had taken place ; these peculiarities are explained by the aid of drawings of the stone after extraction. The third case proved abortive apparently because the treatment was not carried on sufficiently long. In neither of the cases was the treatment carried out as effectively (as the later experience of the author showed) as it might have been. The principal instruction from the cases is, the proof they offered that alkalizing the urine does *not* cause the stone to be encrusted with a phosphatic deposit so long as ammoniacal decomposition of the urine does not take place.

Section 8. Discrimination of the cases in which the solvent treatment is and is not applicable.—The conclusions come to are : That the solvent treatment is inapplicable in all cases where the urine is ammoniacal. When the urine is acid (before treatment) the case is *primâ facie* suitable for the alkaline solvent treatment ; but exceptions must be made of cases where it is known or strongly suspected that the stone is composed of oxalate of lime, also where the stone is large. In cases where the urine is acid, and there is no indication of the nature of the stone, it may be either uric acid or oxalate of lime, or an alternating calculus composed of these two substances. Such cases deserve a trial of the solvent treatment for a limited period of a month or six weeks. The cases which are especially suitable for the solvent treatment are those in which (the urine being preliminarily acid) it is known or strongly suspected that the stone is composed of uric acid, and has not yet reached any large size.

Section 9. Directions for carrying out the solvent treatment effectually.—The urine must be kept *continuously* alkaline, and alkaline to a mean degree corresponding with the maximum solvent powers of solutions of carbonate of potash. The treatment must be given up immediately if the urine become ammoniacal.

Section 10. An examination of some of the objections which have been urged against the principles of the solvent treatment.

The appendix to the first part contains some experiments showing that the cystine is even more amenable to the alkaline solvent treatment than uric acid.

The second part of the paper contains three sections.

Section 1 contains experiments on the solvent treatment of uric-acid calculi by injections into the bladder. Solutions of the following substances were tried in a manner to imitate injections into the living bladder : bicarbonate and carbonate of potash, common phosphate of soda, basic phosphate of soda, borax, borax with liquor sodæ, potash soap, carbonate of lithia, liquor potassæ, and liquor sodæ. The results obtained demonstrated conclusively that their

operation was so slow that no practical advantage could be obtained from their use.

Section 2 records some experiments on the effects of a solution of carbonate of potash and dilute nitric acid on oxalate-of-lime calculi: neither solvent promised any useful result.

Section 3 shows the unsusceptibility of phosphate calculi to solutions of the alkaline carbonates. Brodie's method of injecting dilute nitric acid into the bladder was imitated in one experiment, with results confirmatory of his statement respecting the use of this treatment in phosphatic concretions.

ART. 127.—*Calculus Diseases in Russia.*

By Dr. KLEIN.

(*Medico-Chirurgical Review*, Oct., 1864.)

Dr. Klein observes, that while so many statistical accounts have been published in France, England, and Germany, with respect to the prevalence of calculous affections in these countries, and the results of the different modes of treatment, little beyond mere hearsay has transpired with regard to Russia. Whatever may be the cause of this, it has not arisen from want of material; and having resided for several years as assistant in the Moscow Surgical Clinic, the author is desirous of contributing something towards supplying a desideratum. He believes that in no country is lithiasis more prevalent than in the centre of European Russia, the dwellers in the upper region of the Volga being especially liable to it. The northern and southern portions of the empire furnish a smaller contingent, while in the western provinces the disease is rare. The hospitals of the great towns, especially those of Moscow and Kasan, are the great resorts of these patients; so that, for example, at certain times of the year, a fifth part of the entire number of the patients of the Moscow Surgical Clinic consists of stone cases, more than sixty of these per annum applying for treatment. The great bulk of these persons belong to the class of country people, children being three times more numerous than adults, while so rare is the disease among females, that of 1792 patients treated during the years 1822-60, only four were females. The composition of the calculus is usually compound, pure uric acid or oxalate of lime calculi being rarely met with. The nucleus is usually composed of uric acid and its salts, which is succeeded, in the great majority of cases, by oxalate of lime, while the phosphates, often forming the almost entire calculus in other cases, furnish only the outer layers. In Russia, uric-acid calculi are found much less often than in the rest of Europe, the oxalates and phosphates being much more frequently met with. It is to be presumed that the almost exclusive use of vegetable food and sour drinks by the peasantry may contribute to the prevalence of the oxalates, although that other unknown causes are in operation is seen by the fact of the disease being met with soon after birth and during lactation.

It often happens that adults only apply to the hospital when the disease has become too advanced to admit of an operation being performed, and even in children the stone has frequently been allowed to attain a large size. Chiefly in consequence of this delay in seeking relief, lithotomy has usually been the operation resorted to. Unfortunately, the statistics of the Russian hospitals have not as yet been published, and the author is obliged to confine himself to a statement of the results obtained at some of the Moscow hospitals, as reported by Dr. Bassoff, or observed by himself. These figures are, however, larger than those published in English or French treatises, and refer to 2968 cases treated in 1804-41, and to 1518 treated in 1822-60, making a total of 4486 cases. In the first series of operations, there were 2694 recoveries and 274 deaths; in the second, 1240 recoveries and 278 deaths—i.e., a total of 4486 cases, with 3934 recoveries and 552 deaths. This amount of success, which the author has good reason to believe has been also attained in other Russian hospitals, is somewhat superior to that obtained by English and French operators, and is in a great measure due to the large proportion of children which furnished the cases operated upon. Lithotrixy, up to 1860, has been performed in the Moscow Hospital upon 222 patients. In 24 cases it had to be supplemented by lithotomy: 19 of the patients dying and 5 recovering. Of the other 198 cases, complete recovery took place in 167, and 31 proved fatal. The proportion of deaths (1 in 6.35) was greater than that attendant upon lithotomy (1 in 8), the greater ages of the patients submitted to it having, however, to be borne in mind. In 62 cases, occurring in patients from one to fifteen years of age, 6 only terminated fatally. In 24 cases, a single *séance* suffices for the removal of the entire stone, which in several instances measured eleven lines. As to the cause of death after lithotrixy, this almost always arose from acute kidney disease and its consequences, cystitis being seldom met with, while in patients dying after lithotomy, urinary infiltration, cystitis, peritonitis, and pyæmia were the usual occurrences. Kidney disease, then, should be considered as an almost absolute contra-indication to lithotrixy, as even the gentlest manipulation may then be followed by the worst consequences. In the Moscow Clinic, 405 patients were operated upon for stone during 1849-59. Of these, 293 were children (1 to 15 years of age), and 112 adults (15 to 65)! Lithotrixy was performed 30 times in the first category of cases, and 55 times in the last; so that of 10 children, 9 were operated upon by lithotomy and 1 by lithotrixy; and of every 2 adults, 1 underwent lithotomy and the other lithotrixy. Lithotrixy was also performed upon four females with success.

ART. 128.—*On the Treatment of Stricture of the Urethra by Subcutaneous Division.*

By Dr. HENRY DICK.

(*Proceedings of the Royal Med.-Chir. Society, May 24, 1864.*)

In the year 1853, Dr. Henry Dick published his first case in the *Medical Times and Gazette*, and in 1855 he sent a memoir to the Académie de Médecine in France, in which two other successful cases are related. Since that time, Mr. Wm. Adams and Dr. Dick's colleague at the National Orthopædic Hospital—Mr. Allingham—have operated after the same method with the best result. Dr. Dick divides strictures into two classes, after their physical properties—namely, into dilatable and non-dilatable. Stricture may occur at any spot of the urethra; but the most frequent is the bulb. They are less frequent at the fossa navicularis and the membranous portion. Stricture is the result of inflammation, a new tissue being formed at the strictured spot, which is of a fibrous nature. The greatest number of strictures take the form of atrophy; but a few are met with of the hypertrophic form. In drawing attention to the shape of the stricture, Dr. Dicks points out that every portion of the stricture must be divided, because if only the narrowest part be divided symptoms of stricture will return. He further alludes to deviation of the urethra in strictures, believing that the back opening of the stricture does not correspond with the front opening. He says that those pathological changes are the result of post-inflammatory retraction. Dr. Dick passes in review the different treatment of stricture. He believes dilatation by the graduated metallic bougies is the safest; but there are cases where dilatation will not give much relief to the patient, or sometimes social exigencies urge the patient to get radically cured. The different methods employed he divides into three: 1st, cauterization; 2nd, splitting or tearing; 3rd, cutting strictures. And the cutting he subdivides into three kinds—the internal, external, and the subcutaneous methods. He thinks cauterization the most objectionable, having regard to the pathological anatomy of strictures. Splitting he only admits in a few exceptional cases—where division by the knife cannot be practised with safety, where a number of strictures are closely following each other, or where a large part of the urethra is strictured; but even in these there is no certainty if the stricture has been really torn or only forcibly dilated. He cites two case of sudden death occurring after splitting. His other objection to splitting is that the pain is so violent that recourse must be had to chloroform. Besides, it is a principle in surgery never to tear parts when they can be cut with safety. Dr. Dick thinks the internal incision is the most logical, having regard to the pathological anatomy of strictures; but its execution has great drawbacks: he alludes to the difficulty of making the cut at the right spot with the instruments. Incisions with those cutting machines are very difficult to execute, as very

after the skin has been drawn outward, a string is inserted under the part to be removed. Dr. Jones is of the opinion, however, as being matter is removed, the string is not to be removed. The stricture is then divided, and the stricture is then divided in the membranous portion, and the stricture is then divided in the urethra. The patient is then placed in position, the conducting catheter is introduced until the two knobs stop before the stricture; then the surgeon, by skilful manipulation, slides out the small grooved conductor, which was concealed in the conducting catheter through the stricture. The conducting instrument being then in position, the surgeon delivers it into the hand of his assistant, telling him to keep it gently but steadily against the stricture. He then feels outside the urethra for the two small knobs, grasps with his left hand the penis with the instrument, and places his thumb just before the knobs, having his index and middle fingers at the back of the penis; he then takes the tenotome in his right hand, and thrusts it between the two knobs, pushing it resolutely through the stricture, and divides it in that *sawing* manner in which usually tendons and fibrous tissues are divided. He thinks the cut should always be from three quarters of an inch to an inch long; also that the knife should not be withdrawn until the surgeon is quite convinced that the stricture is completely divided. The conducting catheter is then withdrawn, and lint and sticking plaster placed on the external wound, and the whole kept in position by a T-shaped bandage, a common roller, and a few pins. The patient is then put to bed, and his urine drawn off twice or thrice a day when required with a large catheter. Dr.

Dick strongly objects to leaving a catheter in the urethra after the operation. He now quotes four cases of his own and two of Mr. Allingham's, all of which were attended with the most successful results. In them he relates as a remarkable fact that shivering always took place, but no bad results followed. The only case in which shivering did not occur was that after incision in the fossa navicularis. Another point of importance on which he dwells is, that dilatation with a large metallic bougie should be practised once a week for six months after the operation.

ART. 129.—*Case of Traumatic Stricture of the Urethra cured by Subcutaneous Division.*

By Mr. WM. ADAMS, Surgeon to the Orthopædic Hospital, &c.

(*Proceedings of the Royal Med.-Chir. Society, May 24, 1864.*)

This case is given in a note appended to the paper by Dr. Dick, noticed in the preceding article. The operation performed, which resembles very closely subcutaneous tenotomy, is that proposed by Dr. Dick.

CASE.—In June, 1862, C. W——, aged twenty-six, an officer in the army, first consulted Mr. Adams on account of a very severe stricture of the annular or ring-like form and gristly substance situated in the anterior portion of the urethra, two inches and a quarter from the external orifice. The stricture, which could easily be felt by external examination, had been caused by the bite of a horse in India a year and a half previously, and the inconvenience now suffered by the patient incapacitated him for military duty. At the time of the accident both the scrotum and penis were much injured; profuse hæmorrhage from the urethra occurred, and a portion of the mucous membrane is said to have protruded from the external orifice. Abscess formed and opened externally close to the frænum, where a fistulous opening communicating with the urethra remained at the date of Mr. Adams's operation. Ever since his recovery from the immediate effects of the injury, the patient had been obliged to wear, night and day, a short catheter of small calibre (about No. 2); the disposition to contraction being so great that if this were discontinued more than a few hours, he had the greatest difficulty in passing a tube, even of less diameter than a No. 1 catheter. Mr. Adams considered that an operation offered the only means of permanent cure; but at the patient's request this was postponed, and gradual dilatation tried by a gradual series of short silver catheters. The patient left town, and persevered in his treatment for eight months, wearing a catheter day and night. Dilatation could not be carried further than to admit a No. 4 catheter, which was habitually worn, when Mr. Adams was again consulted in February, 1863. The disposition to contraction was so great that when the No. 4 catheter was left out during the night, only a No. 1 or No. 2 catheter could be passed in the morning. Mr. Adams now urged an operation; and, thinking that any attempts at forcible dilatation, such as Mr. Holt had recommended, would probably fail in consequence of the large size and unusual gristly induration of the stricture, determined to adopt the subcutaneous division of Dr. Dick, more especially as he had assisted Dr. Dick in two operations of this kind in severe cases, in both of which the operation had been eminently successful.

On the 16th of March, 1863, Mr. Adams performed the operation, with the assistance of Dr. Dick. The instruments used were the tenotomy knife, and Dr. Dick's grooved staff with a bulbous extremity, having within it a smaller grooved staff, which can be passed through the stricture, and form a director, along which the knife can be passed in dividing the stricture, when the bulb of the larger staff has been passed down to the stricture as a guide for the introduction of the knife. In performing the operation, the tenotomy knife was passed through the skin externally, directly into the groove in the bulb of the larger staff, and thence onwards along the groove in the smaller staff through the stricture; then, leaving the groove, the knife was directed outwards towards the skin, dividing freely the stricture and some of the corpus spongiosum a little above and below it. In the present case no difficulty occurred in the operation; but, as a complication, two other strictures were discovered by the bulb of the larger staff—one an inch and a half from the external orifice (*i.e.*, nearly an inch anterior to the main stricture), and the other more than an inch behind the main stricture; so that in order to divide these, it was necessary to introduce the tenotomy knife in two different places. When the main stricture was divided, the tissue gave way very much like a tightly-stretched tendon, and could be both heard and felt.

Immediately after the operation, in which very little hæmorrhage occurred, a No. 12 (English) catheter was passed into the bladder without any difficulty, and left in for a short time, slight pressure on the penis being kept up. The catheter was not left in during the night. The next day a No. 12 catheter was introduced twice, and the urine drawn off. This was continued day after day two or three times, and on the fourth day the patient was indiscreet enough to walk down to his club and dine with some friends. Not feeling so well afterwards, he remained two days in-doors. On the eighth day he went out of town. He was able to pass a No. 10 catheter for himself without any difficulty, and this he was directed to do at first twice and then once a day. The No. 12 catheter seemed to meet with a little obstruction at the seat of the deepest stricture, or a little beyond this, but passed readily through the situation of the main stricture. On the 15th of April the patient was carefully examined by Mr. Adams, and no disposition to re-contraction existed. He was improved in every respect. On the 8th of June, less than three months from the date of the operation, Mr. Adams reported this gentleman as fit for active military duties. He was now directed to continue passing the No. 10 catheter twice a week and then once a week, which was to be gradually discontinued. It may be safely affirmed that in this case no better result could have been obtained by any other method of treatment; and it certainly offers encouragement to test further the advantages of the subcutaneous division of stricture, and determine the cases to which this method of treatment is especially applicable.

ART. 130.—*On Two New Specific Remedies for Gonorrhœa.*

By DR. THOMAS B. HENDERSON.

(*Glasgow Medical Journal*, April, 1865.)

The first of these remedies is the oil of yellow sandal wood. It is obtained by distillation from the wood of the tree *Syrium myrtifolium*, of the genus *santalum*. It grows in the East Indies. One pound of the wood yields two drachms of the oil. Lindley writes,

"This oil is said to be used to adulterate the oil of roses." Professor Redwood in his supplement to the Pharmacopœia, on the authority of Dr. O'Shaughnessy, writes, "Sandal wood in powder is given by the native physicians in ardent remitting fevers. With milk it is also prescribed in gonorrhœa."

"In my experiments with this drug," says Dr. Henderson, "I have found it perfectly innocuous even in large doses. From 20 to 40 minims three times a day, diluted with three parts of rectified spirit, and flavoured with ol. cassiæ or ol. cinnam. is the ordinary formula I employ; water and a confection after. In cases of the disease at the first, second, or third stage, in susceptible persons, I have often seen the most marked suppression of the discharge within forty-eight hours. It has the great advantage of being a pleasant medicine, not liable to cause sickness, agreeable to the taste, and grateful to the stomach. It is a medicine as to efficacy, in my opinion, equal, and frequently superior, to bals. copaib. or cubeb pepper. I have often succeeded with it, when both had been fairly tried, and failed. Besides, it is convenient and portable; and if the patient is delicate, or in bad health, or the system disordered, the possession of a remedy which will act as a stomachic medicine, and cure the disease, is, I think, to be highly valued. I have used it in many cases during the past five years. I have no theory to offer as to its mode of acting. My experiments have been numerous, but entirely of a practical character. The odour of the drug is slightly perceptible in the urine. Its action on the urethra is observed, in susceptible cases, within a few days after beginning its use. Almost every druggist keeps it for perfumery purposes."

The other remedy recommended is the Gurjun or Gurgina balsam, or wood oil. It is the product of the *Dipterocarpus turbinatus*, an immense tree growing in different parts of India. Incisions are made and heat by fire is applied to the root. One tree yields about 40 gallons in a season; distilled with water, it yields 35 per cent. of volatile oil. Wood oil is a liquid of the consistence of olive oil, of a dark reddish colour and slight odour. Pereira gives a good account of this medicine when speaking of the adulterations of bals. copaib. In the new edition of Royle's *Materia Medica*, p. 319, it has the honour of occupying one line. Referring to the products of the dipterocarpæ, it is written, "There is a wood oil which contains a principle analogous to copaiba." In the other works of *materia medica* it is either not mentioned or only slightly noticed. The description of this medicine which caused the author to try it is contained in the *Manual of Practical Therapeutics*, by Mr. Edward John Waring.

Mr. Waring says:—"It might be advantageously introduced into English practice as a cheap and efficient substitute for Copaiba. The dose is 10 to 15 drops, thrice daily."

Dr. Henderson continues:—"It is now several years since I commenced to experiment with wood oil. I have only used it in cases where copaiba had been fully tried and failed. In every case it was successful within a week. No symptoms of inconvenience in any of the cases were produced. I gave it in what may be called

large doses, a teaspoonful two or three times a-day, *uncombined*. I have not been able to investigate its action any further, as my supply became exhausted; and it is not easily procured in this country. I am thoroughly convinced it is an excellent medicine."

ART. 131.—*On the Use of Permanganate of Potassa
Injections in Gonorrhœa.*

By Dr. J. G. RICH, of Roachville, Canada West.

(*Canada Lancet*, July 15, 1865.)

Dr. Rich says that he has frequently employed, during the past two years, the permanganate of potash as an injection for gonorrhœa, and with the most satisfactory results, in some cases having effected a cure in forty-eight hours.

His usual mode of treatment is as follows: "R.—Potassæ bitart. ʒj; Podophyllin. gr. j.—M. In chart. No. IV. divid. S. One every two hours until free catharsis is produced.

"After which, R.—Potassæ permangan. gr. vj; Aquæ fontan. ʒj.—M. S. To be used as an injection three times a day.

"I direct at the same time the free employment of mucilaginous drinks, as althæa, ulmus, acacia, &c., and put the patient upon a non-stimulating regimen.

"Out of sixty-four registered cases this course of treatment has failed in but two instances. And I find that recent attacks usually become arrested by it after from three to six injections. I have found it advisable to continue the demulcents for at least a week after the cessation of the discharge. In none of all these cases was the injection continued after the fourth day.

"When accompanied by chordee, I usually employ the following: R.—Lupulin, ʒjss; Pulv. camphoræ, ʒj; Micæ panis, q. s.—M. Ft. mass. in pilulas xvi, dividenda. S. Two, three, or four on going to bed."

(C) CONCERNING THE UPPER EXTREMITY.

ART. 132.—*A Case in which the Ends of a Divided Median Nerve were successfully United by Suture.*

By M. NELATON.

(*Gaz. des Hôpitaux*, No. 40, 1864.)

At a recent sitting of the Société de Chirurgie of Paris, M. Honel related at length the following case by M. Nélaton.

CASE.—On the 24th of April, 1863, Nélaton removed a necroma from the inner and upper part of the left arm of a woman twenty-four years old. The tumour being pressed forward, and an incision made over the skin covering it, the nerve trunk implicated—the median—was very carefully

laid bare. The nerve was then seized by forceps below the tumour, divided, and a silver wire passed through the lower portion. It was divided in a similar manner above the tumour, and the wire also passed. The tumour was removed, and a second wire was carried through the cut extremities of the nerve. The wires were then very carefully and gradually tightened, so as to bring the cut surfaces into contact, and then secured. After the operation the patient could move the ring and little fingers perfectly well; but the thumb, index, and middle finger were immovable; and there was complete loss of sensation in all the parts supplied by the median nerve. On the fourth day after the operation there was much pain in the thumb and two first fingers, and the latter could be flexed a little; but opposition of the thumb was still impossible. The collateral nerves of the palmar aspect of the thumb and two first fingers were insensitive to touch, the branches on the back of the two last phalanges insensitive, those of the first phalanx sensitive in some degree, those of the external aspect of the thumb somewhat more so. On account of the severe pain the wires were removed under chloroform; but one of the loops was left behind. Eight days after the operation the pain had disappeared; and the patient could easily and quickly flex the thumb and two first fingers, and could oppose the thumb. Microscopic examination of the tumour showed on one surface of it a portion of nerve completely uninjured; while on the growth itself were many fibrils unhurt, and many completely degenerated.

ART. 133.—*Partial Dislocation of the Right Forearm forwards, with Fracture of the Inner Condyle.*

By Dr. J. MARIT.

(*Rec. de Mém. de Méd. ec., Milit.*, Août, 1864; and *Schmidt's Jahrbücher*, No. 3, 1865.)

CASE.—The patient, when tipsy, fell downstairs. He struck his elbow against the edge of a step, and observed at the time that he was unable to move his forearm. Two days later the neighbourhood of the joint was much swollen, the skin reddened, the forearm in a state of half flexion, and any attempt to move it was extremely painful. A comparison of the two members, placed in the same position, showed that the right was elongated; and that the arm was somewhat flattened posteriorly, and prominent on the inner aspect below. The point of the olecranon was deeper and further forward than natural, and above it could be felt the outline of the condyles of the humerus, and the empty *cavitas olecrani*. In front, on account of the flexed position, the deformity of the joint was less apparent. In pronation and supination, the rotation of the head of the radius could be plainly felt, but somewhat lower down than natural. It was remarkable that the head of the radius was apparently close to the radial head of the humerus, and was only separated from it by a small transverse furrow. At the bend of the arm was an elevated fold, caused by the biceps tendon, which was tense and somewhat oblique in direction. At the upper part of the forearm, on the outer side of the biceps tendon, was a depression; on the inner side a pointed process, the coronoid process of the ulna. The point of the inner condyle could not be felt; but further forwards than its natural place was a movable piece of bone. Of the triangle with its apex downwards, formed by the olecranon and the two condyles in flexion of the arm, only the outer condyle was in its proper place. The olecranon was displaced downwards and forwards, the internal condyle forwards and inwards; and, on replacing

the latter, marked crepitation became manifest. It was supposed that the patient had fallen upon the olecranon, and produced the dislocation by direct violence, and had also struck the inner condyle against the edge of a step, producing the fracture. This was distinctly limited to the condyle itself, and did not extend to the trochlea.

Reduction having been easily effected, the arm was put into two bent pasteboard splints; the inner of which was furnished with an opening for the internal condyle. After eight days, careful passive movements of the joint were made; and, after forty days, the patient was discharged cured.

Dr. Strabel (*Schmidt's Jahrbücher*) observes of this case, that it is unique. He observes also that, since the arm was elongated, and the point of the olecranon was resting upon the trochlea, while at the same time the head of the radius remained almost in contact with the radial head of the humerus, it follows that the forearm must have received a considerable inclination outwards: a point not mentioned by Dr. Marit.

ART. 134.—*A Case of Dislocation of the Carpus forwards.*

By Dr. THOMAS ROAB.

(*Rec. de Mém. de Méd. éc., Milit.*, Août, 1864; and *Schmidt's Jahrbücher*, [No. 3, 1865.]

CASE.—A man, forty-five years old, emaciated, and the subject of congenital syphilis, came into hospital on account of very extensive syphilitic contraction and ankylosis of several joints. This yielded to careful treatment, the prolonged use of the iodide of potassium and iron, &c. &c., so that after six months the patient could move about on crutches. The left wrist, however, did not recover, and examination showed that it was suffering, not from syphilis, but from the results of former injury. The patient stated that thirty years before, he fell upon ice upon the back of his left hand, that the pain was very severe, the hand immovable and distorted, but that no attempt was made to replace it. The hand swelled greatly, and the pain continued for several days; and six months elapsed before there was the slightest movement of the fingers.

When examined, the left forearm was found prone, and could not be supinated. The back of the hand was turned backwards, and the hand could be extended to an angle less than a right angle, but only so far flexed as to make the metacarpal bones parallel with the radius and ulna. The antero-posterior diameter of the wrist was increased; the transverse diameter unchanged. The circumference of the left wrist, immediately below the styloid process, exceeded that of the right wrist by two centimetres. The hand was slightly adducted; the fingers were capable of considerable flexion and extension movements; but the first phalanges were flexed at right angles to the metacarpal bones, and could only be a little extended by force. The thumb was strongly adducted, and drawn in to the palmar surface. On following the course of the bones of the forearm from above downwards, a projection could be clearly felt at the back of the wrist joint, a projection bounded on both sides by the styloid processes. Under this projection was a depression, recognisable as the concave articular surface of the radius. Over the prominence, the extension tendons were strained as tense cords, but admitted of being moved a little laterally. The styloid process of the ulna was more prominent than that of the radius. The front aspect of the

wrist joint presented a bony prominence, somewhat more elevated than the dorsal one, but with an outline less clearly distinguishable; the tendons of the palmaris longus and flexor carpi ulnaris could be discovered traversing it. Measurement of the two arms showed about a centimetre of shortening.

This dislocation is one the occurrence of which was doubted by Dupuytren; and, although several cases have been recorded, it is still extremely rare. It may be doubted whether the original injury was attended by fracture; but, as no deformity or shortening of the bones of the forearm was present, it is most probable that the dislocation was uncomplicated.

(D) CONCERNING THE INFERIOR EXTREMITY.

ART. 135.—*A Case of Bony Anchylosis of the Hip-Joint, in which the Neck of the Femur was divided in order to form a False Joint.*

By Mr. BRODHURST, Assistant-Surgeon to the Orthopædic Hospital, &c.

(*Medical Times and Gazette*, Feb. 4, 1865.)

Cases of bony anchylosis are rare. When the disease is seated in the hip-joint the patient is very helpless, and can only move by the aid of crutches. He is less helpless when any other joint is affected. The propriety of interfering with bony anchylosis of the knee or ankle joint may be questioned, but in the case of the hip and the elbow joint it is of great importance to give the patient a chance of renewal of motion, even where motion would seem to have been hopelessly lost. In operating it is important to divide the bone as near as possible to the articulation. In the elbow a wedge-shaped piece may be taken from the centre of the articulation; and in the hip the neck of the femur may be divided just below the head of the bone. The divided ends of the bone may then be scooped out, so that both surfaces shall be concave. There is difficulty in retaining motion in these cases, so strong is the tendency for bony union to occur. A swinging limb need never to be feared. If the action of the muscles cannot be gained, reunion by bone is certain to take place. It is important, therefore, to divide the bone in the most favourable position for the action of the muscles, and that point must be the nearest possible point to the articulation itself. In these cases we have to deal with tolerably healthy structures, and hence it is that the tendency to repair is strong in them. The muscles, too, which formerly moved the limb are somewhat altered in structure, and through disuse they will have lost power. It will require, therefore, for a lengthened period both patience and fortitude to gain fair muscular power after bony anchylosis has once become fully established.

CASE.—A. M., aged twenty-three, suffered from bony anchylosis of the left hip. When she was ten years old she met with an accident, through

which inflammation was excited. She continued to walk, however; no attention being paid to the limb for many months. She limped as she walked. The limb swelled; an abscess formed, and continued more or less to discharge pus, with portions of necrosed bone, for ten years. Pain and abscesses at length ceased; and the limb became motionless. The author first saw the patient in 1862; she was in fair health. The question simply was, Could motion be given? Anchylosis had taken place without dislocation of the head of the femur occurring. The neck of the bone was in part absorbed. The limb was shortened one inch and a-half. The pelvis was rendered oblique—apparently increasing the shortness of the limb by two inches. She had during the previous year walked with crutches, and worn a boot which was raised three inches in the sole. The case appeared favourable for operation, and it was thus performed:—An incision three inches long was made commencing over the head of the femur and passing to the outer side of the great trochanter; from the upper angle of which another incision extended inwards for two inches. The neck of the bone was divided, and the ends gouged out as before described. The flesh wound healed almost by the first intention. Movement of the limb was attempted when the cicatrix had formed, but it was difficult, and so painful that without chloroform it could not have been borne. This passive motion was, however, continued, and at length the limb moved readily, and even some voluntary motion was gained, so that the patient could flex the limb to a right angle. After six months she could rotate the limb outwards, and sit down at ease. The pelvic obliquity was easily removed; the horizontal position being in itself almost sufficient for this purpose; and the foot was consequently brought by so much nearer to the ground. A steel support, with joints opposite to the hip, knee, and ankle, was fitted to the limb; and the buttock was supported by a leather shield. With this instrument, and a couple of sticks, the patient moved about easily. The operation has now been done two years. There is no lack of firmness about the hip joint; but, on the contrary, it requires constant exercise to keep it free. The patient now walks without the instrument, and with one stick for support.

ART. 136.—*Adjustment of a Foot-Board to Liston's Modification of Desault's Long External Splint in the Treatment of Oblique Fractures of the Femur.*

By Dr. JAMES STANNUS HUGHES, Surgeon to Jervis-street Hospital, &c.

(*Dublin Medical Press*, Jan. 1865.)

"Having frequently, in the treatment of oblique fractures of the femur in adults, felt the want of efficient means for steadily maintaining Liston's modification of Desault's long external splint (by far the best appliance in such a case) in its proper position without the necessity of too frequent a re-adjustment of the bandage, or the disadvantages of interfering with either the patient's sound lower limb or bedding, I have," writes Dr. Hughes, "lately used in Jervis-street hospital, in a case of unusually oblique fracture of the femur occurring in the person of a very strong muscular young man, the result of a railway accident, a very simple, inexpensive, easily procured, and, as far as I know, novel contrivance, by which all the in

dications for the successful carrying out of extension by means of Liston's splint—namely, the steadying of the splint in the vertical direction—the prevention of either aversion or inversion of the foot—the relief of the heel from pressure—the non-interference with either the sound limb or bedding—the non-necessity for a too frequent re-adjustment of the retentive apparatus—the power of varying the irksome position of the limb—and, finally, the facility of introducing the bed-pan when necessary, without disturbing the fracture, were so perfectly accomplished, that I consider it my duty to bring the subject before my professional brethren with as little delay as possible.

“The apparatus (first constructed for me in its simplest form of light deal wood by Mr. Wm. Dudley White, the resident apothecary of Jervis-street hospital, and since improved on under my directions by Messrs. Thompson and O'Neill, surgical instrument-makers to that institution), consists, as may be seen by the accompanying woodcut, by Mr. Oldham, of a movable foot-board attached by means of hinge-rings and screws to the posts at the foot of an ordinary iron hospital bedstead. In the foot-board, which is six inches in depth, and made of light American ashwood, there is a slot three inches deep, into which the lower end of Liston's splint is received, by which arrangement the splint becomes, as it were, a part of the bedstead and not of the bed—cannot wobble—does not interfere with either the sound limb or bedclothes—relieves the heel from pressure—admits of the introduction of the bed-pan, with the assistance of the central hand-bar or bed-pull, without disturbing the fracture, and demands but occasional re-adjustment, thus carrying out all the indications in the treatment of an oblique fracture of the femur in a more complete manner than by any other plan I am aware of, and which is, as far as I know, *original*—a term, however, that I use with some degree of diffidence, as there are few surgical appliances, if any, which can, in my opinion, in the present day, be truly so named.

“The foot-board can be reversed so as to answer either thigh bone when fractured.

“In addition to the slot, I have had two vertical mortises, two inches in length and half an inch in breadth, made in the foot-board opposite the sole of the foot of the injured limb, in order that, if from any cause in any given case it may become advisable to maintain extension by long and broad strips of adhesive plaster from the leg (so strongly recommended by Gross, Neill, Gilbert, Swinburne, and other eminent American surgeons—a method I have occasionally adopted with advantage), instead of by means of the figure-of-eight bandage from the foot, they may be readily brought out through the mortises, and tied on the outside of the foot-board and lower end of the splint.

“It is almost unnecessary to state that the foot-board, and together with it the lower end of the splint, can be raised and lowered at the will of the surgeon by means of the ring hinges and screws—advantages of no little importance, looking to the comfort of the patient.

"In conclusion, I wish to observe that, in oblique fractures of the femur, in conjunction with Liston's long external splint and the new foot-board, I use, as many other surgeons are in the habit of doing, an anterior scored splint reaching from near the groin to the knee, and likewise both internal and posterior thigh splints made either of sole leather, gutta percha, or bookbinder's pasteboard of proper consistence, which materially assist in keeping the fragments in their proper position, and thereby prevent the possibility of angular displacement."

ART. 137.—*New Mode of Amputating at the Knee.*

By Dr. LÜCKE.

(*British Medical Journal*, Oct. 29, 1864.)

At the recent annual meeting of the Association of German Naturalists and Physicians at Giessen, the section on surgery had a lively discussion on the merits of a new mode for amputating the thigh at the knee, excogitated by Gritti; in it the thigh-bone is sawn off through the condyles, or at the epiphysal line, and the anterior flap is allowed to retain the patella, which it is intended to heal upon the sawn surface of the femur. Dr. Lücke had done this operation in four cases. The first was that of a soldier, who had received a shot into the knee at Missunde; he died in the second week, of purulent discharges: the patella was not united to the femur. The second case dated from the storming of Düppel. Here the patella became firmly united with the saw-cut on the femur, and the patient had an excellent stump; the cicatrix was behind, and had not to sustain any pressure during walking on the stilt-foot. The third and fourth cases both ended fatally. Dr. Lücke communicated another case, from Rotterdam, in which the patella had been perfectly united with the section of the femur. Professor Wagner, of Königsberg, next detailed the result of the discussion of a case of Gritti's operation, which had recovered, but died subsequently of kidney disease. The patella was riding upon the anterior edge of the cut surface of the femur, was thickened and bent, and united to the femur by connective tissue only. Professor Bardeleben, of Greifswalde, preferred amputation in the lower third of the femur to Gritti's operation. Dr. Heine had collated twelve cases of Gritti's operation, made during the last campaign. Two only were successful: one was the case of Lücke, already mentioned; the other, a case in the Austrian hospital at Altona, which had been operated upon immediately after the sea-fight near Heligoland. All the others died of pyæmia.

ART. 138.—*A Case of Complete Dislocation of the Tibia forwards.*

By Professor W. LINHART.

(*Wien. Med.-Halle*, 1864.)

The injury was caused by a mass of stone falling upon the limb. Reduction was effected without difficulty under chloroform; the limb was placed on a metal splint, and iced compresses applied to the knee for the first few days. The case did well; and in three months the patient was able to walk easily, and to flex the knee-joint to a right angle.

ART. 139.—*Resection of One Inch of an Imperfectly-united Tendo-Achillis, and Successful Treatment by Suture.*

By Dr. WARREN WEBSTER.

(*American Medical Times*, Sept. 3, 1864.)

CASE.—The subject of this operation was a half-breed Indian, who had ruptured the tendo-achillis in running a foot-race. No regular treatment had been practised. When the patient was seen by Dr. W. "there was an intervening gap between the divided ends of about an inch in length, where but little plastic matter seemed to have been poured out to fill up the space. The uniting bond was so elongated and weak as to render the limb powerless in progression. I resolved to expose the parts by a free incision, remove the slight connecting medium, pare the retracted extremities, and endeavour to unite them by the introduction of sutures of silk. The operation of bringing the severed ends in contact, after the removal of the intervening substance, was attended with considerable difficulty. This, however, was accomplished by placing the limb in a thoroughly relaxed position, and inserting two strong ligatures through the ends of the tendon about three lines from the extremities. The parts were thus approximated, and the relaxed position of the limb was maintained by an apparatus consisting of a ring of leather placed round the thigh, above the knee, from which a cord was attached to a loop in the back of a slipper. The gastrocnemii muscles were also surrounded by a firm bandage. This apparatus was used for six weeks, when the patient was allowed to walk about, wearing a high-heeled shoe for three weeks longer. After having tied the ligatures, one end of each was cut off, and the others withdrawn, as practised in the ligation of vessels. The incision was then united its entire length (which was about three inches) in the most exact manner possible. The ligatures were removed on the twenty-fifth day, and during the greater part of that time the wound discharged purulent matter. The fourteenth week after the operation the patient walked with scarcely any lameness, and the tendo-achillis appeared to be perfectly united."

PART III.—MIDWIFERY.

MIDWIFERY AND DISEASES OF WOMEN AND CHILDREN.

(A) CONCERNING PREGNANCY AND PARTURITION.

ART. 140.—*On the Position of the Uterus.*

By Dr. M. CLAUDIUS, Professor of Anatomy at the University of Marburg.

(*Medical Times and Gazette*, Jan. 7, 1865.)

If we seek an answer in literature to the question regarding the position of the uterus, we obtain everywhere a definite one. Anatomists, gynæcologists, obstetricians, and surgeons alike, in Germany, England, and France, express themselves with rare concurrence to the effect—that the uterus in its normal position is surrounded before and behind by the intestines; in other words, that the Douglassian space is filled up with loops of intestines. In older anatomical works this is in general not directly asserted, but simply implied; for instance, in the frequently-recurring statement that the uterus and broad ligaments form a transverse septum through the small pelvis, and such like. In recently published works, on the contrary, there are the most definite statements on this point. Any observations or reasons on which this opinion rests are in the majority of cases not communicated, the fact alone being set forth as undoubted and generally believed.

“On the contrary,” says Dr. Claudius, “the investigation on living persons with healthy uterus, and on the dead subject, furnishes totally different results. In the case of the former, the finger introduced into the rectum always feels the probe lying in the uterus, without intestinal loops being noticeable between. And in the dead subject the uterus with the broad ligaments and ovaries is observed in by far the greater number of cases lying as close to the posterior wall of the pelvis as the lungs are to the ribs. The rectum generally passes close behind the left border of the corpus uteri, swerves, however, also, sometimes considerably towards the left, more rarely towards the right, so that the uterus lies in the middle line. The

broad ligaments cling so very close to the posterior wall of the pelvis, that in the case of sagittal sections (through frozen corpses) which pass between the uterus and ovary nothing of them is noticeable at first inspection, and it is only discovered after more careful examination that a thin double flap of peritonæum lies against the wall of the pelvis, in whose upper end the section of the tube is visible. The ovary lies more laterally—its long axis horizontal or generally a little raised at the outer end—in a shallow cavity, the fossa ovarii, which is hollowed out in the tissue, that fills up the upper border of the musc. pyriformis, the hole intended for the vasa and nerv. glutæus superior. The anterior side of the ovary is completely covered by the ala vespertilionis, so that the intestines do not come in contact with it at any point. The Fallopian tube takes its course close above the upper border of the ovary, and then curves downward behind its outer end, so that the infundibulum is enclosed in the lateral half of the fossa ovarii between its base and the posterior side of the ovary. This at least I observed in a case in which there seemed to be nothing abnormal.

"The main points of the above description rest on examinations of 88 subjects, 36 of which were sections of frozen corpses represented in Pirogoff's *Anatomia Topographica*, while four were in Le Gendre's *Anatomie Chirurgicale Homolographique*. I have to thank the friendly exertions of Dr. Füngel, Director of the Hospital at Hamburg, for 38 cases. Lastly, I myself have examined 9 subjects, 5 of which were sections of frozen corpses. 10 cases must be excluded from this number, because the uteri showed evident traces of disease or malformation. But of the remaining 78 cases the genital organs lay 70 times close upon the posterior wall of the pelvis, whereas in 8 cases loops of intestines lay embedded in the Douglassian space.

"The last-mentioned cases are, in my opinion, likewise of a pathological nature. The uterus is, then, only in its normal position when along with the broad ligaments it touches the posterior wall of the pelvis and the rectum. In my opinion, it is always a case of anteversio, anteflexio, or antefractio uteri when intestinal loops are present in the Douglassian space. This opinion is founded principally upon two reasons.

"Firstly, because in this manner alone the uterus can obtain a safe position in the pelvis. This argument will probably be much attacked. We find in all authors this alone impressed, that the uterus is very movable, but that is no reason why we should suppose it is capable of movement. To be sure, it is undoubted that the uterus can easily be moved by violent treatment through moving of the neighbouring parts, but nevertheless certain that it rests in the healthy female quite unmoved in the pelvis. This is sufficiently proved by repeated explorations *per rectum et per vaginam*. If we except the rectum, which during filling and emptying moves the uterus gently forwards and backwards, there are no causes of disturbance at all. The above statement is sometimes grounded on erroneous observations. If a softened vaginal portion may be moved easily in all directions by the finger introduced into the vagina, it is

concluded from this that the uterus makes the contrary movements between the intestines in the pelvis, and therefore forms a double-armed lever, whose hypomochlion lies in the bottom of the pelvis, fascia pelvis, and musc. levator ani. It is easy to comprehend that this is impossible. For not only would every considerable movement of the uterus—by which the intestinal loops changing their position would be forced out of the space above the bladder into the Douglasian space, and *vice versa*—cause great violent twitching of the broad ligaments, such as no living being could bear, even although we assume the mobility of the intestinal loops to be very considerable, but also, on account of the round ligaments, the lateral flexures would be much sooner limited than the forward movements, which is not the case. Certainly the body of the uterus can only be moved by the finger, introduced into the vagina through the raising of the bottom of the pelvis. To be sure, this appears to possess in different individuals a very varying resistance. From the rectum the uterus may be pressed as far forwards as the anterior wall of the rectum, and the point of fixture of the uterus yield. It is true that by means of the uterine probe the uterus can be moved forward and towards both sides, but this violent treatment has nothing to do with the solution of the question, whether the healthy uterus ever changes its position during life? No circumstance speaks to the affirmation of this question, while the examination of healthy individuals and of corpses necessitates the admission that the uterus has a seat as well protected against the intestines as the liver has. The unvarying position of the vaginal portion and its resistance in healthy women to the pressure of the finger can only have their cause in the steadiness of the body of the uterus in the small pelvis. Nor is the uterus more affected by the emptying bladder. The plicæ vesico-uterinæ and the tissue lying beneath are, according to examinations on recently killed animals, so lax and yielding that the uterus cannot be drawn by them forward against the intestinal loops. Moreover, experiments on corpses, undertaken with all necessary caution, prove that the bladder can be filled and emptied without the uterus being moved in the least.

“The uterus is mainly held in its position by the round ligaments. After these have reached the lateral wall of the pelvis, having left the inguinal canal, and continuing along the posterior wall of the pelvis, in a curve concave forwards and downwards, they attach themselves firmly to the uterus, below the insertion of the Fallopian tubes. In this position they are tense, and permit of a lateral motion of the uterus as far only as their own substance may be stretched. Of themselves alone they would be unable to fix the uterus, because they would cut through between the intestinal loops. This is hindered by means of the broad ligaments, whose use consists in this, that they form broad folds about the round ligaments, by means of which these gain resistance against the intestines. This apparatus can only then be effectual when the broad ligaments touch the posterior wall of the pelvis with their whole surface. If there are intestines in the Douglasian space, the uterus is moved forward, and the round ligaments lose their tension; the uterus is therefore no

longer fixed, either laterally, forwards, or backwards. It will be moved to all directions by the intestines, whose state of repletion alters several times daily. If the intestines in the Douglasian space are being filled, while those lying above the bladder are being emptied, the uterus will be anteverted, but in the contrary pressed backward, along with which lateral inflexions generally will be combined. If the intestines are being filled on the one side before on the other side, behind the broad ligaments, the uterus must necessarily be turned on its axis. All these movements must be accompanied by distensions of the broad ligaments; and thus the many complaints of those affected with anteversio uteri are rendered comprehensible.

"It is easy to understand that as long as the uterus retains its normal rigidity, intestines can never enter the Douglasian space. The pressure which is exercised on its whole anterior surface by the intestines has the result of retaining it in close contact with the rectum, and must completely overcome the gentle pressure that is exerted by parts of single loops of intestines on the superior opening of the Douglasian space. The intestines cannot enter at the side of the uterus, behind the Fallopian tubes, on account of the narrowness of these, just as little as on the borders of the omentum majus. It is not impossible that single loops of intestines, which have passed into the Douglasian space, can be expelled out of it by the overpowering pressure of the filled portions of the alimentary tube, as soon as the former have become more movable by being emptied.

"During pregnancy the uterus touches the posterior wall of the pelvis, as far as the promontorium, and remains there during the involution post partum.

"The uterus can maintain its position against the intestines only as long as it retains its normal rigidity. As soon as its tissue is relaxed, and therefore the pressure on the under part of the anterior surface is not more continued as far as the upper border, it is possible for the intestines to inflect it from above forward and backwards. These physical conclusions confirm the statement of pathological anatomists, that softening of the tissues of the uterus is the principal cause, in the most cases, of deviatio uteri.

"According to the aforesaid, the apparatus of the uterine ligaments is only then useful and effectual when the uterus reclines against an unyielding surface, and loses instantly its whole meaning if intestines are behind it.

"The second reason for my opinion seems to be of still greater weight. If intestines lie behind the uterus they will generally touch the ovarium and the infundibulum of the Fallopian tubes. The fimbriæ are so movable, light, and tender, that it is impossible to conceive their lying safe between the intestines, both as regards their structure and position. They will be removed from the ovary, and the tender fringes covered with ciliated epithelium will be destroyed. We can understand this if we think of the lumps into which the omentum majus is rolled up, if it chances to come between the intestines. There would be no longer the possibility of the entrance of an ovum into the infundibulum. But simply the contact

of the ovary with intestines would render the conveyance of a discharged ovum in the infundibulum impossible. Through their changes of form and position the ovary, which is held fast at one end by its ligament, would be tugged up and down, and be thus removed from the infundibulum. The discharged ova must fall into the furrows which run in the most different directions between the close-lying intestines, and through their peristaltic movements be conducted anywhere but into the infundibulum.

"If, on the contrary, the ovary with the lateral half of its posterior side resting on the infundibulum lies in its fossa, protected in front by the ala vespertilionis against every touch, the entrance of an ovum into the infundibulum is conceivable. The ova expelled from the follicles are forced by the pressure exerted on the anterior side of the ovary into the canals, which lie upon the upper and lower borders of the ovary, as the pressure there must be less. The canal of the upper border, threesided in a transverse section, is formed by the passage of the ala vespertilionis from the ovary to the posterior wall of the pelvis; those of the under border are small slits before and behind the ligamentum ovarii. They are stretched a little by the liquor folliculi, and the supposition that a stream is excited in them by the ciliated tissue, powerful enough to conduct the liquid with the excessively slippery ovulum into the infundibulum, has nothing absurd in it. It is possible that the pulsation of the branches of the art. hypogastrica, which have their origin directly below the ovary, assists the passage of the ova. We may expect that the special examination of the uterus *in situ* will shed a light upon this process. The examination of the epithelium in the fossa ovarii of a person who has died suddenly during ovulation is very desirable, as, perhaps, ciliated epithelium is developed there on the peritonæum during this time (as in frogs).

"It is easy to comprehend from the above that sterility must, as a rule, be united with anteversio uteri. An intestinal loop, indeed, may lie behind the uterus, and still the situs ovarii be quite normal, as I have observed in one case. On the other hand, Dr. Füngel has informed me that in two cases where the situs uteri was normal, he saw the infundibulum hanging down before the ovary between the intestines,—an anomaly which likewise must entail sterility. Sections of female corpses of the rapidly-disappearing savage tribes in America and Australia would be interesting, since it is not impossible that epidemical deviations of the uterus are the cause of the sterility of the females.

"The comparative anatomy and the history of development show us likewise the female genital apparatus in close contact with the rectum and the posterior wall of the pelvis. In the first months of fetal life it is impossible for intestinal loops to enter into the Douglasian space on account of their thickness. In the most Mammalia the central portion of the uterus bicornis touches the rectum.

"I lastly take the liberty of calling the attention to some conclusions immediately connected with the aforesaid. If a gynecologist says, 'through its great mobility the uterus can be brought by double manual exploration into every situation, and

every change of position can be caused artificially ; indeed, a soft uterus can be inflected,—the moral thereof needs not to be explained. But the uterine probe might, in the hands of a careless physician, become the most dangerous instrument. If the handle of the probe lying in the uterus is but gently moved backwards, the intestines must fall into the Douglasian space, and the consequences thereof will certainly be mischievous."

ART. 141.—*On some of the Early Signs of Pregnancy.*

By Dr. ROUTH, Physician to the Samaritan Free Hospital
for Women and Children.

(*British Medical Journal*, November 26, 1864.)

The remarks which follow are from a paper read at Cambridge at the thirty-second annual meeting of the British Medical Association:—

"The signs of the first period of pregnancy—i. e. from the date of conception to the fourth month—have always been a source of some difficulty. The principal symptoms, which need here only be mentioned, because they assist mostly the diagnosis, are, cessation of the menses ; the purple colour of the vagina ; the peculiar character of the urine, which contains kystine ; the velvety feeling of the uterine cervix ; the enlargement of the breasts and womb ; with the peculiar character of the areola.

"Excepting the presence of kystine, the collection of which is sometimes not practicable, and an occasional appearance in the breast-areola, where white follicles containing a white secretion, of about the size of the heads of pins, are congregated in large numbers near the nipple, and extend outwards—a state of things which, I believe, is never found out of pregnancy—all the other enumerated symptoms may be due to other causes. The menses, we know, will cease from a variety of diseases. Patients with any uterine tumours, particularly older women and those who have piles, and varicose veins on the legs, may have the purple colour of the vagina. The velvety feel of the uterus may be perfectly imitated by an ulcerated os. The breasts may enlarge, and the areola of the breast may look very suspicious from mere uterine irritation. The womb may become hypertrophied from congestion. Upon the appearance of none of these can we implicitly rely, though in their general coexistence we have more confidence. It is for this reason that I have thought an additional symptom, which can be in most instances detected, might form the subject of an interesting communication ; and as such I have ventured to bring it before your notice.

"Hitherto much reliance has not been placed on *auscultation* in the first period of pregnancy. The so-called placental souffle, if heard at all, has been heard quite at the end of this period. Dr. E. Kennedy of Dublin, it is true, has given some examples in which he

was able to hear it with certainty at the twelfth, eleventh, and even in one instance at the tenth week; but his experience has not been confirmed by others. Nägele states that it is only heard after the fourth month. Rigby admits that it can be detected as early as the fifteenth or sixteenth week. Murphy states that it is not generally heard before the fourth month.

"Years ago, Dr. Rigby premised that there was every reason to suppose that it might be heard at a still earlier period, if the uterus were at this time within reach of the stethoscope. The object of this communication is to show the means by which I have attained to this end; and henceforward, I imagine, it will be in the power of all occasionally to foretell pregnancy by this symptom as early as from the seventh week to the ninth, probably even from the sixth, when the placenta is situated near the cervix. At any rate this is the earliest period at which I have heard the distinct soufflé. At this early date, however, the sound differs somewhat from the placental soufflé, as heard at a later period. Nor is it very easy to give an accurate description of it in words. It is more like the sudden cessation for a moment of a general splenic murmur. If I may so express it, it is a sudden interruption of an intensely vesicular and muffled murmur, synchronous with the pulse. This it is which makes it very difficult to catch sometimes, and which requires the closest attention and a most practised ear to make it out at all. I have also observed that the soufflé, when it does occur at a very early period, and when very distinct, has somewhat a high pitch; so far confirming the opinions enunciated by authors, who have described it as assuming a piping character when the placenta is attached near the cervix. Curiously enough, in those cases where I have heard this high-toned pitched murmur *per vaginam*, I have heard it very early also in the iliac region of one side.

"It is but right, however, to add that, as with ordinary auscultation-sounds heard over the abdomen, so with these pelvic sounds, they may be absent altogether. Sometimes we may lose them at a sitting, and are not again able to find them. Thus much, however, I think we may safely conclude, that during the first period, and in a doubtful case, no examination is complete without the use of the *vaginoscope*.

"The cases now related amount to nine,* in which the pregnancy was detected as early as from the sixth to the thirteenth week. All but one were married, and therefore there was no object in deceiving me; and as I saw them all purposely and by special appointment, to make out whether pregnancy existed or not, no difficulties were put in my way in the examinations made.

"Of course, these do not include all the cases which I have examined or noted particularly from the sixth to the thirteenth week: but they are fair samples among several others, a few of which I regret I have been obliged to set aside, as I omitted to note at the time the date of their first examination by me. In these also, however, the pregnancy was made out very early. I have named

* These cases are to be found in the paper under consideration.—Ed.

these several instruments *vaginoscopes*. Perhaps, were I to adopt the plan followed in the words *stethoscope* and *ophthalmoscope*, it would be better to call them *coleoscopes*, using two Greek words instead of a compound of Latin and Greek. The examination of the uterus was made with these instruments introduced within the pelvis in other ways. Thus a flexible tube passed up *per anum*, with an ear-piece at the distal end, answered for auscultation. This method, though, as it might be supposed, useful, was sufficiently disgusting to discourage any further attempts. I have also attempted to hear through the sacrum; but the indications here were very uncertain. Stethoscopy *per vaginam* is not new. I am told that the late Dr. Stroud had devised a plan to effect this, and, if I mistake not, some German author; but I have not yet been able to obtain their memoirs. An examination *per vaginam* does not, however, in these days, entail the same reprehension as formerly. Thus my facilities have been greater.

"If a woman be placed on her side, with her legs very much flexed upon her abdomen, then auscultation may be readily made with a common stethoscope, provided the distal end be small, by pressing directly with it against the perinæum. This, by the apposition of its internal surface, impinges against the vagina and uterus, and so we can often hear sounds originating in the latter organ. Where, however, the woman is fat, and the perinæum thick, the sounds are muffled. In this case, it may be passed *per vaginam*, and made directly to impinge upon the uterus. With a glass syringe, in doing this, as the rubbing of the clothes is not heard, no exposure whatever is needed. The same auscultation may be made with the woman on her back, with a curved glass speculum.

"It would be quite a little history, were I to enumerate the several plans I have adopted in the hope of devising a more perfect instrument for the purpose. I have tried metal stethoscopes, bent at a right angle, sometimes using a thin membrane at the vaginal end, of India-rubber. I have used metal tubes, with a membranous sac in the interior, so constructed that I might blow air into it at will, and make the sac more or less tense. I have used wooden vaginoscopes, with a membrane interposed, at the union of the vaginal with the external portion of the stethoscope, which was here enlarged so as to form a cavity of greater dimensions than on any other part of the stethoscope. This cavity I have made exactly elliptical, so that the sounds conveyed would be concentrated at a focus, and be carried thence by a tube to the ear, upon the principle of Mr. John Marshall's ear-trumpet. The object of the membranes and membranous sacs was twofold: to reverberate, and thus intensify in the cavity the sounds, as is the case with the tympanum. Secondly, the small apparatus with which it was connected, and by which it could be tightened or slackened at will, was to alter the pitch, so that sounds might be heard which the uterus might convey, on a membrane as tense as would produce the note conveyed by the womb-sounds. This, however, in practice, was found in no way to act as I expected. Sounds appeared to be equally well transmitted without as with the

membrane, while a double membrane seemed rather to muffle the sound than otherwise.

"I have even used a metal vaginoscope with fluid compressed within it by a membrane, to try if I could feel thereby any wave of pregnancy or fluctuation conveyed.

"In the present stage of my inquiries, I have come to restrict myself to two varieties—the glass stethoscope, and the wooden vaginoscope.

"There is nothing new in the first. It has been used for a long time; and glass has a signal advantage. It is a beautiful conductor of sound, and rather, I think, raises the pitch. At any rate, it enables you to hear more distinctly than with the ordinary wooden stethoscope. Add to this, it is very readily kept clean, and does not make any noise when rubbed against the clothes. This, as I before said, is a great advantage, and allows it to be used very modestly; and when manufactured of the shape I employ—that is, long, and with a curve—is very easy of application.

"The second variety includes the single and the double vaginoscope. You will see that the vaginoscope, as I use it, is nothing more nor less than either the single or double wooden stethoscope, with elastic tube, *to the distal end of which is affixed a wooden speculum*. Occasionally I use a solid tube in lieu of an elastic one, made either of gutta percha or of wood. I am satisfied, however, that, were this manufactured of glass, it would be preferable.

"These, then, are the instruments. One word now about their application in diagnosis.

"My first business was to ascertain what were the normal pelvic sounds heard in a non-impregnated woman. There may be none. Occasionally, where there is much uterine congestion or vaginal inflammation, you may hear a regular pulse. This is however rare, and is probably the vaginal pulse. Commonly, you hear the gurgling in the intestines, passages of wind like bronchial respiration, and of fluid. These sounds, curiously enough, I have not heard in pregnancy, except occasionally the pulse before referred to. I imagine this to be due to the non-transmission of sounds through fluid, which the uterus contains in the early as well as in the more advanced periods of pregnancy.

"The murmur of fibrous tumours presents some resemblance to that of pregnancy. The true *fibroid murmur*, however, is tubular, and usually louder, and often accompanied with a thrill, and a single or double cardiac sound, synchronous with the first or both sounds of the maternal heart. The *placental murmur* is intensely vesicular, without thrill, and usually without the cardiac sounds conveyed from the mother, except under very exceptional circumstances."

ART. 142.—*On Excessive Sickness in Pregnancy.*

By Dr. HENRY BENNET.

(Lancet, Jan. 7, 1865.)

Writing from Mentone, in answer to several communications on this subject, which had recently appeared in the pages of the *Lancet*, Dr. Henry Bennet says:—

“According to the experience of my entire obstetrical career, extreme, intractable sickness during pregnancy is generally occasioned by the antecedent existence of inflammatory mischief of the uterus, or of actual chronic inflammation of the body or of the neck of that organ. Women who have suffered from and have been cured of uterine inflammation, a short time before becoming pregnant, nearly always have laborious pregnancies. They suffer greatly from sickness, from uterine, ovarian, and dorsal pains, and from hysterical and neuralgic symptoms. That such should be the case is but natural. A great and trying physiological task is imposed on an organ only recently cured, yet tender. Is it surprising that it should perform its functions with difficulty, and react painfully on sympathetic organs? Still worse is the case of the patient who becomes pregnant whilst actually labouring under chronic inflammation of the body of the uterus or of its neck. In the former case the pregnancy is not only laborious, but is very often brought to an early and premature close. Inflammation of the neck is not attended with quite so much danger as regards the existence of the fetus, but may render the life of the mother one of anguish and suffering, especially from constant and extreme sickness, if the pregnancy is prolonged. Many young women marry actually suffering from inflammation of the uterine neck, which marriage aggravates. If pregnancy occurs, they may become the victims of excessive sickness. Many women who have had children again become pregnant whilst under the influence of some cervical lesion, laceration, inflammation, or ulceration, the result of their last confinement. Is it surprising that they should suffer from an unusual amount of sickness?

“From what precedes, it must be evident that the duty of the obstetrician in a case of obstinate and dangerous sickness during pregnancy, which resists medical treatment, is to examine his patient, and to ascertain the state of the uterine organs. If he finds, as I have constantly found, actual inflammatory disease, his duty is to treat it. What can sedatives, and medicinal agents in general, poured into the stomach, do for inflammatory and ulcerative disease of the uterine neck? In such cases a few touches of nitrate of silver and an astringent injection will arrest sickness that has baffled the skill of half a dozen medical men, and the resources of the Pharmacopœia. I have thus saved the lives of many children, and I verily believe of some mothers. I have heard of cases of death from sickness, in which no examination as to the existence of

uterine mischief was thought of, but I have never known of any in my own practice, consulting or private, nor have I ever been obliged to artificially bring on abortion as recommended by obstetric authorities in extreme cases. Nearly all my very severe cases have been cases of uterine disease, and have yielded to the proper local treatment of the disease, not to medicine.

"After all, the above facts are merely the extension to the pregnant state of our present knowledge of uterine pathology. One of the commonest symptoms of uterine inflammation is nausea and sickness. In such cases the only treatment to be relied on is that of the uterine disease. To illustrate this fact, I may mention that even in this little Mediterranean nook I have now two lady patients, consumptive mothers of young families, who came to me suffering from constant sickness, which was supposed to be the result of the cough. I soon found that it was in reality uterine, the result of ulcerative uterine disease, connected with the last confinement. I treated at once the uterine mischief, and the sickness is now in both cases steadily declining, and will, I believe, soon disappear."

ART. 143.—*The Management of the Third Stage of Labour.*

By Dr. H. EASTLAKE.

(*Proceedings of Obstetrical Society of London*, November 2, 1864; and
Medical Times and Gazette, December 3, 1864.)

This paper consists of an historical, analytical, and critical dissertation on this subject. Having briefly described the opinions which existed amongst the accoucheurs of times gone by, the author proceeds to give an account of the modern views and principles which govern the placental stage of labour. Dr. Eastlake lays great stress upon the hand being placed firmly on the fundus uteri at the moment the child is being expelled, the uterus being thus followed down, and the contraction maintained by gentle pressure. He states that external manipulation, judiciously applied, was, in the majority of instances, quite sufficient *per se* to effect the expulsion of the afterbirth, without any traction whatever on the funis. He believes that the great secret is to exert the pressure during a contraction; in short, to act in unison with nature as we did in the application of forceps, where we applied our chief force at the moment of a pain. Dr. Eastlake says he had no doubt that many would imagine that, after all, this was no modern idea; but he demonstrates that this teaching was not definitely described and insisted on in our manuals of obstetrics. Dr. Credé, the Professor of Midwifery at Leipsic, appeared to be the only one who had advocated this doctrine and brought it prominently before the profession. The author next considers the subject of retained placenta, and alludes to the various causes which arrest Nature's process of extruding the afterbirth. The three steps in the natural expulsion

—namely, (1) the detachment from the wall of the uterus, (2) its extrusion from the uterine cavity, and (3) its expulsion from the vagina—are duly recognised and dwelt upon. Regarding the subject of morbid adhesion of the placenta, Dr. Eastlake throws out a suggestion as to the possibility of being able to diagnose this condition by means of auscultation. He reasons by analogy in stating that for a long time he had been fully persuaded that by means of auscultation we often possess not only a negative but a positive sign of foetal death. He describes a peculiar modification of the uterine *souffle*, which to his ear was very characteristic when foetal life had been extinct for any time. The alteration in tone suggested the idea of a muffled sawing noise, very different to the gentle blowing murmur heard in normal cases, where a living child existed in utero. How soon the modification took place the author is unable to state, from want of a sufficient field for observation. He considers that no ergot of rye should be given in cases of retained placenta, unless we were quite sure that no abnormal adhesion or irregular contractions existed. In cases of spasm of the os uteri, where the placenta became encysted, the administration of chloroform is recommended. Another point of interest alluded to by Dr. Eastlake, bearing upon the subject of his paper, is the occasional existence of a supplemental afterbirth, which was spoken of by Dr. Barnes, Dr. M'Clintock, and other authors, under the name of *placenta succenturiata* or *placenta spuria*. Dr. Eastlake has seen a specimen of such an afterbirth in the museum of the Lying-in Hospital in Dublin, obtained from an ovum of five months. When such a mass remained in the uterus after the true placenta had been expelled, it often gave rise to secondary hæmorrhage, and an impression arose that due caution had not been exercised in the extraction of the afterbirth. He agrees with Dr. M'Clintock that, remembering the possibility of such an occurrence, we should be slow to utter any opinion which would damage the character of a professional brother. In conclusion, the author alludes to the several conditions which generally authorise us to have recourse to a speedy removal of the placenta, such as post-partum hæmorrhage, convulsions, rupture of the uterus, and possibly, under certain circumstances, where the uterus was inverted, with the afterbirth still adherent.

ART. 144.—*Notes on some points in the Treatment following Severe Labour.*

By MR. GEORGE K. H. PATERSON, Balbeggie, Perth.

(*Glasgow Medical Journal*, January, 1865.)

In general obstetric practice it is often witnessed, that certain cases of abnormal labour make slow recoveries. To find out the why and the wherefore of this will now and then perplex both the student and young practitioner. Unquestionably, not a few tedious

labours, unaided by the forceps, will be followed by more or less exhaustion. But, in addition to this, we frequently find post-partum hæmorrhage occurring at the termination of severe labour.

On other occasions, again, when the labour pains happened to become, during and towards the last stage, unusually strong and severe, or where the strength of the patient had been wasted to no purpose, through the unwise advice of over-meddlesome attendants, flooding, as might have been anticipated, resulted as a general rule. How often, moreover, on the expulsion of the child or placenta, or when the medical attendant is in the act of applying the binder, is the occurrence of hæmorrhage recognised by the well-known "gush," or from the suddenly blanched appearance of the patient. If the uterus be not now induced to contract sufficiently, by appropriate means, not only will the recovery of your patient be tedious, but in a large majority of cases she will be in considerable and immediate danger. Mr. Paterson writes:—

"Having been called during the past years to a number of cases of severe labour at the full time, where the labours lasted for twenty or thirty hours, and which were complicated by post-partum hæmorrhage, I found, that of all the usual external uterine applications previously used by me, external compression of the uterus was by far the most effectual. This I performed in the following manner:—The patient having been gently turned upon her back, I placed one of my hands upon each side of the abdomen, and compressed the uterus several times towards the centre of the hypogastrium, and in a downward direction, until the uterus was felt to be firmly and unmistakably contracted. By this method, when gently and perseveringly exercised as long as there is necessity for doing so—coagula, either in utero or in the maternal passages, are readily, and often speedily and successfully, expelled; simultaneous contraction of the uterus ensuing. Some medical men may, nevertheless, say, in opposition to such, that pressure on the uterus with only one hand, followed by the pad and binder well tightened, serves equally the same purpose, or if not, passing the hand at once into the uterus, dashing cold water, opening of windows, &c.; and affirm, that one or all of such means, if used in succession, will bring about contraction of the uterus. My own experience, however, has convinced me that in such cases the economy of time is of paramount importance, and that the readiest method for, as well as our only security against post-partum hæmorrhage, consists in a properly induced and sensibly felt contraction of the uterus. The actual condition of the latter should be carefully ascertained in every case before leaving the patient. A binder and pad cannot be expected to do much, and is as little to be depended on, unless the uterus has previously contracted sufficiently, either of itself, or by the effect of the aforesaid method. The practice of passing the hand into the uterus is not always advisable in post-partum hæmorrhage; nor in the majority of cases is it productive of such an amount of success as to justify its adoption.

"Profiting by those cases, and the success resulting from uterine compression, I have now for a considerable time past pursued this

practice in labours of every kind, immediately after their completion; and I am confident that I have thereby ensured greater safety, reduced the intensity of after-pains, and prevented unnecessary uterine hæmorrhage, even in cases where flooding had in previous confinements been wont to occur.

"Besides, and apart from the *rationale* of the above method, its availability after the completion of labour certainly speaks in its favour. In many cases, when we are called upon to treat post-partum hæmorrhage, the more ordinary remedies may be quite out of our reach; whereas, compression in the manner I have indicated is always available, and by saving time will often turn the scale in favour of our patient.

"Before summing up these few remarks, let me add my humble testimony to the good effects of a more supporting diet after childbirth, as for a series of years past I have enjoined and ordered a pretty liberal diet, especially in great exhaustion from uterine hæmorrhage after severe labour; and instead of harm, much benefit was derived by the patient after delivery. I am convinced that by this means recoveries will be less tedious and far more satisfactory, than under the frugal bill of fare which in such cases is usually prescribed."

ART. 145.—*Retention of Urine mistaken for Labour.*

By MR. R. L. JOHNSON.

(*Dublin Medical Press*, March 15, 1865.)

CASE.—Mrs. T.—Forty hours previously symptoms of labour had commenced, and had, since that time, increased hourly. The patient was thirty-five years of age, and of a weak habit of body. She had had four children, all of whom were alive, and were strong and healthy. She complained of periodic "bearing-down" pains, accompanied by a discharge; she could not walk, nor lie on either side; and the only relief she experienced was when on her back and with her extremities semiflexed, but in this position she could not for any length of time continue on account of "shooting-pains" in her back and a sense of suffocation. On examination, Mr. Johnson found her pulse 90, her tongue typhoid, her skin hot and clammy, and during pains copious perspiration visible on her head, face, and palms of her hands. She slept but little. She could form no opinion as to about the time of her expected confinement; but she considered her size to have increased very much within the last few days. Her stomach was irritable; her appetite poor; her bowels were constipated; and she had insatiable thirst. Her urine was high-coloured and contained pus; and it was stated therewith that each movement she made, and during the pains, a considerable quantity of urine passed involuntarily from her. The surface of her abdomen was tense, hard, and unyielding; a little below and to the left of the umbilicus there was dulness on percussion, whilst the remaining portion of the abdominal surface was tympanitic. Between each side of the inesian line and the recti muscles, and just above the pubes, were thumb-like prominences, very painful to the touch, and semi-elastic. Mr.

Johnson was not permitted to pass a catheter; whereupon he cautioned his patient to get immediate medical aid, and then left the house. Three hours afterwards he was again sent for, and he then removed from the patient more than *nine pints* of urine. All "bearing-down pains" ceased for three months. Then they returned, and brought with them a healthy female child.

ART. 146.—*On Dilatation of the Os Uteri during Labour by Incisions.*

By Dr. H. HILDEBRANDT, of Königsberg.

(*Königsb. Med. Jahrb.* IV., I. p. 178, 1864. *Schmidt's Jahrb.*, No. 11, 1864.)

The author commences by a brief account of nine labours, in seven of which primiparæ, advanced in life, suffered from rigidity of the os uteri; against which ipecacuanha, opium, poultices, baths, bleedings, and chloroform were all unavailing. Incisions were made, after which all the cases were fortunately terminated. Incisions were also made, with a like favourable result, in one case of convulsions, and in one of prolapsus of the cord.

He proceeds to consider the supposed risks that have deterred accoucheurs from the performance of the operation. It has been feared that the pain of incisions, in a part already irritated by foetal pressure, and in persons inclined to nervous disorder by prolonged labour, might be productive of mischief. This fear is wholly groundless; the incisions themselves being scarcely felt by the patient, and the relief actually afforded by them being very great. Others have dreaded an extension of the incisions during pain, so that they might come to involve the substance of the uterus, and produce the fatal effects of rupture. This is visionary. The incisions do sometimes yield a little, but never so far as to reach even the cervical portion of the womb; and the operator, by relieving an impediment to the advance of the foetus, diminishes instead of increases the danger of rupture. Lastly, it has been feared that excessive hæmorrhage might attend or follow the incisions, but this fear is never realized in practice. In cases that require such treatment, the os uteri is morbidly changed, and so bloodless, that the hæmorrhage from the incisions does not exceed a few drops. Where incisions are made into a healthy uterus, in order to effect rapid delivery, the bleeding may be greater, but its source is always accessible, and it may, therefore, always be readily controlled, while, in such cases, which are almost limited to eclampsia and placenta prævia, the danger from hæmorrhage can never be equal to the danger of delay. The operation is chiefly indicated, however, in morbid conditions of the vaginal portion of the cervix, such as rigidity, hypertrophy, and malignant disease. For forced delivery, with a healthy cervix, the incisions should be six or eight in number, and not more than three lines in depth.

ART. 147.—*An Inquiry into the best Mode of delivering the Fœtal Head after Perforation.*

By Dr. J. BRAXTON HICKS, Assistant Physician-
Accoucheur to Guy's Hospital.

(*Proceedings of Obstetrical Society of London*, December 7, 1864; and *Lancet*, January 14, 1865.)

In introducing the subject of this paper, the author says that, notwithstanding the employment of premature labour and version, cases would occur in which it was either necessary or desirable to perforate. He points out that the subject had of late years not received the attention it deserved. He alluded to the disputes which arose upon Dr. Osborn's case of E. Sherwood, when that physician asserted he could draw a child's head through a brim having an inch and a half antero-posterior diameter, by tilting the base of the skull sideways, and concluded that the Cæsarean section might be done away with. The disputes which followed were so acrimonious that the valuable points elicited by Drs. Hull, Hamilton, and Burns were, to a certain extent, lost sight of, at least as far as they were calculated to give any rule in practice. Dr. Burns in particular deduced from his experiments that, the calvaria of the fœtal head being removed, the base of the skull could be drawn down easier face foremost than in any other direction. With this the author's experiments entirely agree. And he pointed out further the advantage of the chin pointing anteriorly during the descent. He further instituted a comparison between the opposing diameters when the face is made to present and the other modes of drawing down the base of the skull. He then proceeds to answer the inquiry: if in cases of extreme lessening of the antero-posterior diameter, it is best to cause the face to present; and if, after simple perforation, it is best to continue vertex presentation, at what degree of reduction of the size of the head do the two presentations cause equal obstruction? This he answers by the results of experiments, which might be thus concisely stated. That, as is acknowledged by all, vertex presentation in natural labour is the best; and that after perforation and evacuation of the brain up to the extent of one-fourth, this rule holds good; yet if the evacuation of the brain and collapse of the calvaria by this means, or by more or less fracturing the bones, be carried to a greater degree, we find that the facial presentation affords the easiest mode of delivery, provided that the mentobregmatic fall beneath the bizygomatic diameter. And, further, that if we remove the whole calvaria, leaving merely the base, and then induce face presentation, taking care that the chin, as it descends, points anteriorly, we diminish to the smallest possible amount, short of wholly breaking it up, the opposition of the head, leaving only from one to one and a half inch in depth to oppose the conjugate diameter of the brim, and from

three to four inches at the outside to oppose the transverse. The author, as practical deductions from these facts, recommends in cases where simple perforation failed, to allow the descent of the head in cases of obstruction,—say above three inches antero-posterior diameter,—to break up purposely and carefully the bones of the calvaria, and remove at least a portion, preserving the scalp as protection to the edges, and then to induce face presentation. That when the diameter was under three inches, then to remove all the calvaria, and then to induce face presentation, taking care to bring the chin forwards, if not already in that direction. Dr. Hicks then points out the facility of doing this with a small blunt hook, which could be readily and without chance of injury passed up to the orbit. The chin he had found had a tendency to point anteriorly upon being drawn down. He then enters upon some useful details, and compares this mode of craniotomy with the cephalotribe. He remarks that by this means, in deciding upon whether craniotomy or Cæsarean sections should be performed, the head is not so much to be considered as the size of the body, in cases of brim obstruction.

The paper is illustrated by eight cases of craniotomy, six of which were required for contraction of the conjugate, and two for obstructions in cavity. In all, the induction of face presentation was attended by instant and complete passage through the obstacle. In some of the cases the shoulder and pelvis of the fœtus gave more difficulty than the head. The paper is accompanied by details of the experiments.

ART. 148.—*On the Comparative Claims of Craniotomy and the Cæsarean Section in a certain stage of Labour.*

By Dr. MURPHY, Professor of Midwifery in University College, London.

(*Dublin Quarterly Journal of Medical Science*, May, 1864.)

The following cases may prove interesting as aiding to solve the important question when the Cæsarean section may be justly adopted. It has been stated by the writer, in his published lectures, that the cases there quoted seemed to establish the rule, "that in the ovate deformity of the pelvis, if the conjugate axis be less than two inches craniotomy should not be attempted, but an effort made by the Cæsarean section to save the child." The cases now brought forward afford additional evidence to prove the great danger of craniotomy when the disproportion is so great, and lead us to doubt the propriety of destroying the child by an operation which seems as likely to destroy the mother.

Craniotomy, as compared with the Cæsarean section, seems to the writer in these cases equally dangerous, and, therefore, he feels it his duty to adopt the latter operation as being the best means of saving, at least, one life.

The cases now quoted were brought into University College Hospital, it might be said, at the eleventh hour, to have the Cæsarean section performed; but in both cases the children were already dead, and, therefore, craniotomy was preferred as being generally considered less dangerous to the mother; but had the children been alive the writer would have felt perfectly justified in having them removed by the Cæsarean section.

CASE 1.—M. A., a dwarf, four feet one inch high, was taken in labour of her first child on Tuesday, December 22nd, at about 12 o'clock in the day.

The midwife who was engaged to attend was at once sent for; she came about 3 o'clock, tried her pain, and said that she thought all was going on well. She left, and was again sent for at midnight, but, being engaged with a labour, she did not arrive until 3 o'clock on Wednesday morning.

December 23rd.—The midwife remained until 3:30 in the afternoon, and finding no advance she sent for advice. A medical practitioner arrived, who, finding a difficulty, left the patient, about 5 o'clock, to obtain a second opinion. Having succeeded, both gentlemen remained with the patient until about 8 P.M., and then left her. The patient's friends, who were, no doubt, anxious and indignant, stated "that from 8 o'clock on Wednesday evening until past 11 on Thursday morning they neither heard nor saw anything of the doctors."

The medical practitioner in attendance then arrived, with another gentleman, who, having made an examination, had the patient removed from her bed to a table, where she remained for about twenty minutes, and was then replaced. This, perhaps, was for the purpose of a more accurate examination, but the result was that she was sent to University College Hospital about 5 in the afternoon, in order to have the Cæsarean section performed.

Messages were sent to the writer, and Mr. Marshall, surgeon to the hospital, and both arrived about half-past 5 o'clock.

An examination being made, it was ascertained that the os uteri was dilated to rather more than the size of a teacup; was thick and tender, the head protruding through it, with the bones strongly overlapped.

It was evident that the child was not only dead but putrid. The promontory of the sacrum could be felt with the fore-finger easily, proving the greatly diminished space in the conjugate axis.

In consultation with Mr. Marshall I objected to have the Cæsarean section performed to remove a putrid child, and, therefore, undertook the rather arduous task of extracting it by craniotomy. The head was easily perforated, and the brain removed; the parietal bones at once separated and were taken away; the crotchet (Churchill's) was then fixed in the frontal bone, and again in the occipital, acting alternately, so that by great care and equal difficulty the head was brought into the cavity of the pelvis, but no force could advance it farther; at length, after several efforts, the head separated from the neck. I succeeded in bringing down one arm, fixed a tape round the wrist, and hoped by this means to extract. But no; the shoulder was almost pulled out of its socket without effect; I therefore sought for the second arm, and succeeded in getting it down; pulling, then, on both, the body was at length extracted.

The cause of the difficulty was immediately revealed. The abdomen was enormously distended with flatus, occupying the brim in such a manner as to resist every effort to extract the head or the body until both arms were brought down.

This patient was altogether fifty-three hours in labour—very much exhausted and depressed; she had gone through a very severe labour, and had other causes of mental anxiety. She had some bronchitis on admission, but it did not assume a serious aspect until she was delivered. She was immediately given a composing draught.

25th.—The vagina was carefully syringed with decoction of poppies, and the solution of the hydrochlorate of morphia (M. xv.) given in wine every second hour. There was no tenderness of the abdomen, but increased difficulty in respiration. The morphia and wine were suspended, and every means used to allay the difficulty of breathing, but without effect. She sunk on the morning of the 26th.

On examination after death the bronchi were found filled with a great quantity of mucus, the walls of the tubes being highly injected. A section of the lung exhibited some spots of incipient lobular pneumonia. The liver and heart were healthy; no evidence whatever of inflammation either in the peritoneum or the uterus. Both kidneys were flabby and friable, the capsules easily separating; the cortical substance was very wide, the medullary indistinct, and the pyramidal broadly marked, showing evidence of albuminuria.

The pelvis, having the ovate deformity, was carefully measured. The conjugate axis, from sacrum to pelvis, was $2\frac{1}{4}$ inches. The death in this case may be attributed to bronchitis; but occurring, as it did, twenty hours after delivery, it may, perhaps, be better explained as the result of shock to a constitution already diseased.

CASE 2.—H. S., aged twenty-five, a charwoman, of low stature, was taken in labour, Tuesday, January 5, 1864, in the evening, with the usual premonitory symptoms—pains and cramps in the abdomen—which continued, with more or less force, all that night. Severe and regular pains set in on Wednesday, and continued Wednesday night and Thursday, when, at 5.30 P.M., her sister sought for medical advice.

A medical gentleman soon attended, and remained with the patient until about 7 P.M., when he left.

Friday, January 8.—At 3 A.M. the patient's husband went for the practitioner, who at once attended, and remained until 7 o'clock. No progress being made, he left, and returned at mid-day. Finding no difference, he determined on a consultation.

At 4 P.M., two medical gentlemen returned, and, after a consultation, determined on the operation of craniotomy. The head was perforated, and an attempt made at delivery, but, after two hours' unsuccessful efforts, they determined that the fœtus should be removed by the Cæsarean section. For this purpose she was sent to University College Hospital, about 10 P.M.

The writer was sent for, and arrived about half-past 10 P.M. Having made an examination, he found the promontory of the sacrum equally within reach of the fore-finger, as in the former case; the broken bones of the head occupied the brim. The contraction of the brim was apparently the same, and the child dead. He, therefore, could not consent to the removal of a dead child by the Cæsarean section.

The extraction of the child by the crotchet was undertaken. The bones of the head being so much compressed there was some difficulty in getting the crotchet within the cranium; at length it was introduced, and fastened on the frontal bone. Immense force was required to move the head at all, but at length it advanced very slowly. The frontal bone gave way. The crotchet was then fixed on the occipital, and, by pressing the bone with the fingers strongly against the instrument, it held sufficiently long to get the head past the brim of the pelvis. Its further advance was then more easy; but, in

order to make it secure, an arm was brought down, and the child removed. The operation occupied two hours.

The patient, although very much exhausted, bore the operation very well. An anodyne was ordered, and poppy fomentation for the vagina.

January 9, 8 A.M.—The patient slept well; skin, moist; pulse, 170. She takes nourishment (beef tea, milk, wine) well. A bark mixture was ordered, with sesqui-carbonate of ammonia in effervescence.

10.—She seems going on favourably. The surface was sponged over with warm sponges, and the same treatment continued.

11, 4 A.M.—Mr. Roberts, the obstetric assistant, was called by the nurse, who found the patient in a state of great exhaustion. The pulse was very feeble. Stimulants, however, were freely administered, and after some time she rallied. At mid-day she was in a great degree restored, and slept well that night.

12.—She seems much better; the skin is moist; pulse, 140, with some slight delirium, and pain in the abdomen. The lochia are suppressed.

13.—The patient has had some sleep, and seems refreshed. She went on during the day without any unfavourable symptoms. In the evening she was more herself; pulse, 140, soft and compressible; no pain in the abdomen, but some symptoms of bronchitis have shown themselves.

14.—Bronchitis much increased, with great difficulty of respiration, which could not be relieved by all the efforts used. She sunk at 6:30 P.M.

15.—Post-mortem examination.—Lungs (left): bronchial tubes highly injected; small masses of pneumonia throughout the lower lobe. Right: at the base numerous patches of hemorrhagic extravasation under the pleura. Bronchial tubes highly injected; the lower lobe or section showed large masses of pneumonia, which broke down easily under the finger. Intestines much distended with flatus; no sign of peritonitis except in the neighbourhood of the uterus, where a small patch of lymph was found, low down on the left side, and the nearest coil of intestine was united to it by two slight adhesions. The uterus was well contracted and healthy. Pelvis: the conjugate axis, from sacrum to pubis, was $2\frac{1}{2}$ inches.

In this case there was a much more favourable prospect of recovery than in the former. Bronchitis suddenly seized her on the fifth day, and she sunk under it. She might, however, have resisted the attack had not her constitution been previously exhausted by her labour and its results. It occupied, from its commencement on Tuesday, 5th, seventy-two hours. She also underwent, before her admission into the hospital, an operation which is stated to have occupied two hours, and in the hospital two hours more were consumed in her delivery. This was sufficient to produce great exhaustion, and evidence of it was given, before the attack of bronchitis, by the sudden faintness which seized her on the 11th. The contraction of the brim of pelvis was greater than in the former case.

"Both cases," says Dr. Murphy, "prove the great danger of craniotomy when the disproportion is so great, and seem to justify the rule, that when the conjugate axis of pelvis is two inches, or less, the Cæsarean section should be performed *to preserve the child*. In these cases the children were dead, and therefore they were delivered by craniotomy, as being considered a less dangerous operation to the mother. The difference in the danger in such cases is, however, very slight; and, when such is the case we are justified in the endeavour to save the child's life when that of the mother is in such hazard. The danger of delay is equally obvious. If the medical attendant knew, in the first instance, the exact disproportion

tion and its consequences, a consultation would be at once determined upon, and the proper steps taken; but, unfortunately, not knowing the exact disproportion, there is the disposition to trust too much to what 'time will bring forth,' and either operation is commenced a great deal too late."

ART. 149.—*Seizure of the Neck of the Child by the Uterus during the Cæsarean Section.*

By Dr. RADFORD, Manchester.

(*Lancet*, and *Dublin Medical Press*, Nov. 16, 1864.)

"In reading the proceedings of the Obstetrical Society, reported in the *Lancet* of the 22nd October," writes Dr. Radford, "I observed a discussion on a case of Cæsarean section which happened in 1837, and which was found in the papers of the late T. E. Bryant, Esq. Dr. Playfair remarked, 'that in the only case which he had witnessed, a difficulty was met with which he had not seen alluded to in descriptions of the operation, and which certainly had not occurred in the case under discussion, nor apparently in any of those described by Dr. Greenhalgh. In the case in question, the uterus was opened near the fundus, and although no time was lost in removing the child, still the uterine parietes contracted with such rapidity and force that the head was caught in the incision, and some difficulty occurred in extracting it and the placenta.'

"Every cause likely to destroy the life of the infant during the performance of the Cæsarean section deserves to be fully understood by the operator, as the preservation of its life is one of the strongest incentives for its performance. And the chance of seizure of the neck of the infant, and powerful resistant retention of its head by the excited uterine parietes, is not noticed in any of the systems of midwifery extant in this country.

"I have already, several years ago, drawn the attention of the profession to this occurrence. In a case of Cæsarean section performed in 1821, and published first in the *Edinburgh Medical and Surgical Journal*, and afterwards in the *Provincial Medical and Surgical Journal*, vol. xv., page 426, 1851, the 'body' (of the child) 'was extracted with the greatest ease until the shoulders came to pass, when the uterus suddenly and powerfully contracted, and grasped the child's neck and left arm so strongly that it could not be liberated, although great force was used. In another case related in the same volume of the same journal, page 287, a similar event happened, although precautions had been taken to prevent it. The body of the infant was cautiously and expeditiously drawn forth 'until the neck came to pass, which was then firmly grasped by the uterus, and the head thereby detained.' 'The womb had an appearance as if it was indented, and strongly reminded us of its condition in hour-glass contraction.' Dr. Playfair thinks that this inconvenience might be avoided by making the incision nearer the

lower part of the uterus, instead of at the fundus; but in my opinion there must be some other circumstance to provoke this spasmodic contraction; and in the remarks appended to the cases above referred to, I have ventured to attribute it to detachment more or less of the placenta.

"In natural labour we well know that, as soon as the placenta is detached, the energies of the fundus and body of the uterus are aroused, and contraction follows, and is continued until this mass is expelled."

"Analogy led me to form this opinion; and, if true, it is important for the operator to previously ascertain the exact location of the placenta, and avoid if possible either inflicting injury upon it, or causing its separation until the infant is removed. In my obstetric address on the Cæsarean Section, &c., delivered at the meeting of the Provincial Medical Association (now British) at Manchester, September, 1854, I gave a tabulated statement of the position of the placenta in all the published cases in which it was given, and I am led to conclude that my opinion is correct.

"The incision ought to be made neither to extend too much towards the fundus or towards the cervix uteri. The length of the wound, after the contraction of the womb, will be greatly influenced by a judicious incision: if too much into the fundus, the wound will be gaping, and not in a favourable position for union, and if the cut is made too low into the cervix uteri, as this part is more a dilatable, and not so much a contractable, portion of the organ, the wound will be left considerably longer."

ART. 150.—*Fatal Consequences of a Fall during the last Month of Pregnancy.*

By Dr. SCARLAU.

(*Berl. Klin. Wochenschrift*, I. 29, 1864. *Schmidt's Jahrbücher*, No. 11, 1864.)

CASE.—A healthy young woman, twenty-one years of age, fell off a step upon her back, on the 14th of February, 1864. She was in the ninth month of pregnancy. Two days later trifling pains set in, which increased; and, on the 19th, the patient was delivered of a female child. The first days passed over well; but, on the evening of February 22nd, acute pain was felt in the right side. This was relieved by moist warm compresses. The patient lost appetite, had slight diarrhœa, and rigors alternating with heat. On the 23rd of March she died, having had symptoms of pneumonia since the 7th, and having suffered continually from the febrile heat and cold, and from diarrhœa, with much exhaustion and tendency to collapse. Among other post-mortem changes, the right kidney was found adherent to the ascending colon; and, on separating the adhesion, thick greenish pus was discovered. The kidney itself was healthy; and the abscess was in the fat covering its investing capsule.

Professor Martin assumes that, in consequence of the fall, an effusion of blood took place in the capsule of the kidney, and he cites other instances to show that such effusion, in the neighbourhood of the genital organs of a

parturient woman, is extremely prone to occasion dangerous or fatal sup-pururation.

ART. 151.—*Case in which Amaurosis was observed Eight Times in Succession after Parturition.*

By Dr. H. E. EASTLAKE.

(*Transactions of the Obstetrical Society of London, 1864.*)

CASE.—Eliza Tibbey, aged thirty-four, married; the wife of a painter. The patient states that she has had nine children at the full time and no miscarriages. She has always enjoyed good health. In three of her confinements she was attended by medical practitioners, in the other six she was delivered by midwives. As far as I can learn, all her labours have been natural. She has never lost more than the normal amount of blood, and after the birth of her last child (which took place on the 28th of last January), the hæmorrhage was peculiarly slight, according to the account of the midwife who attended her.

On the occasion of her first lying-in, she tells me that she made a quick recovery and nothing peculiar happened, her sight being then perfectly good; but on the second or third day after the birth of her second child, and after all her seven subsequent labours, she has suddenly become totally blind in both eyes, and also partially unconscious; but when her senses returned, the amaurotic condition remained, and on an average has lasted from three to five weeks.

I saw her for the first time on the 31st of last January, three days after her confinement. Her pulse was rather weak, but there was no marked pallor, nor did she exhibit any great signs of debility. Her intellect was certainly clear at that time, though I believe that she had been somewhat incoherent the day before. She assured me that she appeared to be in absolute darkness, and after a few experiments to satisfy my own mind on that point, I was thoroughly convinced that she could neither distinguish any object, nor had she even the perception of light.

I should perhaps mention that she had never taken any ergot; there was no suppression of the milk or lochia, and I carefully ascertained that she had not been subject to any periodical or long-continued discharge which had suddenly dried up. There was no albuminuria. She complained of nothing but her want of vision, and all her other functions appeared normal. Being a patient from the St. Marylebone General Dispensary, I arranged that she should be seen by my colleague, Mr. Zachariah Laurence, whose reputation as an ophthalmic surgeon must be well known to Fellows of this Society. Having first well dilated the pupils by the application of atropine, he proceeded to institute a most careful and complete ophthalmoscopic examination, but the evidence adduced from it appears to be entirely negative, except that it demonstrated the existence of a somewhat contracted state of the retinal arteries; this latter fact, however, being in all cases a question of degree, may be considered comparatively unimportant.

I may add that no strabismus existed, and that no peculiarity either in point of colour, form, size, or consistence, was noticeable in the eyeball. As the patient was by no means a robust or plethoric woman, and, as I have

stated, her pulse being rather weak, I allowed her a liberal diet, and since her getting up she has been taking bark, with mineral acids, wine, &c., with improvement.

Commenting on this case, Dr. Eastlake says :—

“The most remarkable feature in this case seems to be its apparently utter isolation from the various forms of amaurosis attributable to other causes. In the rejection of these, we might infer, by a negative process of reasoning, that the phenomenon was necessarily dependent in some way upon the puerperal condition of the patient. I confess that I should have been induced to incline to this opinion, had I succeeded in discovering a parallel case either in the history of obstetrics or in the modern practice of midwifery. But I have failed to establish a precedent. Dr. McClintock, the late master of the great Lying-in Hospital in Dublin, whose experience as an accoucheur, I need scarcely say, fairly represents the probabilities of such an occurrence in our day, assures me that he has never met with a similar example; and Mr. Wilde, whose experience as an oculist must be very considerable, states that he has never seen a case of this kind.

“The only authors, as far as I have been able to ascertain, who record anything at all relating to this subject, are Beer, in the year 1817, and very recently Dr. Ramsbotham. The former, in the second volume of his *Lehre der Augenkrankheiten*, describes a form of amaurosis which occurs at the commencement of pregnancy and disappears after parturition, but which is always connected with nausea and uncontrollable vomiting; he adds that we must be careful to distinguish this kind of amaurosis from that which arises sometimes during the last months of pregnancy, and is due to the violent and continued congestion of the head, particularly if there is much fecal accumulation in the intestines with constipation. This form of amaurosis generally lasts until the birth of the child, or if the labour is much prolonged, and accompanied with great exhaustion, the blindness he stated is likely to continue. He also relates a case of a young Jewess, who in her first three pregnancies began to grow blind always immediately at the commencement of utero-gestation, and in the third or fourth month she became completely amaurotic, but, on the first two occasions, remained so only until after parturition, but lost her sight entirely after the third confinement.

“Dr. Ramsbotham describes a case somewhat similar, under the head of cerebral affections in pregnancy, in the *Medical Times and Gazette* of March 7th. I will quote that portion of it which has any interest connected with the one I have brought before your notice.

Dr. Ramsbotham first saw the patient on the 14th of June, 1842; she was then in the last month of her pregnancy.

“He says: ‘The earlier months had passed over very well; but, about six weeks before, she began to lose the sight of both eyes simultaneously, and continued to get worse by degrees until my visit, when she was so completely blind that she could only just point out the situation of the window.

“She had not suffered any pain in the head, nor any unusual drowsiness; her recollection was perfect, and she was quite sensible. The pupils were much dilated, the right more so than the left. The pulse was quick and small. She was cupped, leeches, blistered, and slightly salivated, without relief, and continued getting worse until she could not distinguish the brightest sunshine. On the 23rd she began to feel a tingling and numbness in the right arm and leg, without any loss of power, and on the 24th Dr. Blundell met us in consultation. He feared, as I did, that convulsions or apoplexy would occur during her labour.

“He recommended that more blood should be taken by leeches to the temples, and that the mercury should be continued . . . She went into

labour at mid-day of the 28th, and was delivered ultimately the next day by craniotomy, owing to the pelvis being below the average size—the child being putrid—no convulsions occurred. . . .

“He concludes by saying: ‘As far as her labour was concerned, she went on exceedingly well; but the blindness, numbness, and tingling remained without diminution for ten days. After that she gradually began to mend; in a month she could distinguish objects; in six weeks she told me the hour of my watch, and on the 24th of August, she went out of town very weak, but able to stand and walk with assistance, and she had completely recovered her sight. . . . She had one child afterwards, without any return of the symptoms; but as she left that part of the town, I am not aware whether she ever bore another.’

“The details of these cases, it will be observed, do not completely coincide with those which have come under my notice. Whether there may be any analogy between them, and how either or all may be connected, however remotely, with parturition, are points upon which at present I do not venture to theorize.”

ART. 152.—*Case in which the Uterus was Extruded without being Emptied of its Contents.*

By Dr. E. M. JENKS.

(*American Journal of Medical Science*, October, 1864.)

CASE.—On the 12th day of August, 1863, I was called in consultation with Dr. Tefft, of Sturgis, Mich., to a case of childbirth where the woman had been in labour for four days, having had for a medical attendant during all that time a homœopathist. Upon reaching the house we found the doctor there, who wisely informed us before entering the lying-in room, that “the waters had broken some forty hours before,” and, to use his own language, “labour was going along all right until the head came outside; but there was *prolapsus of the vagina*, which would not let the head pass any further. I tried to stretch the vagina over the head, and afterwards put on liniment to soften it, and then tried, but could not get the prolapsed vagina over the head.”

The patient, Mrs. S., aged twenty-three, was in labour in the eighth month of her second pregnancy. Her first labour occurred at full term, being an easy and natural one. Her general health has been good. She was, as might be expected, nearly exhausted by the length of time she had been in labour. I made a vaginal examination, and found the uterus occupying its proper position; the os uteri dilated about one and a half inch in diameter, soft and dilatable; the head was in the first position; the soft parts were dry and quite tender, probably having derived no benefit from the doctor's manipulations or liniment. There appeared to be nothing to prevent ready delivery, except the absence of labour pains and the great exhaustion of the patient. Upon auscultating the patient's abdomen I could not discover any signs of fetal life. By the administration of alcoholic stimulants and ergot, her strength was sustained and labour pains were re-established. I then administered chloroform while Dr. Tefft attended to the delivery of the fœtus. I made no further examination until Dr. T. remarked “that the head had passed the soft parts, but that there was something unusual.” Upon examining I could distinctly feel the head and shoulders external to the

body, yet surrounded with something, I could not at first tell what. Ocular examination revealed the fact that the head and shoulders were protruded, but *still surrounded by the uterine covering*; the os uteri was dilated no more than at the first examination; the foetal head could be distinctly seen; the powerful propelling force of the abdominal muscles was pushing the gravid uterus still further into the world. Yet what added greater gravity to the case was that at this time I could distinctly see the overstrained uterus beginning to tear at the anterior edge of the os uteri. Fearing a rupture of the uterus I immediately encircled the foetal head with my hands to prevent any further propulsion of the gravid uterus, and if possible uterine rupture, and directed Dr. Tefft to reduce the head by craniotomy, which he immediately did, and by means of the crotchet easily delivered the child through the circle made by my hands. Not until the afterbirth was removed by the hand was the uterus returned to the pelvic cavity. I then kneaded the abdomen and felt the uterus contract to the usual size after childbirth. There was no excessive hemorrhage, and no further unpleasant symptoms. Convalescence was early established, and the patient recovered her full quota of health and strength.

Upon inquiring into the history of this case I learned that at different times during her pregnancy the uterus had been prolapsed, and that at one time she had bruised this dependent tumour; an abscess followed which suppurated and discharged for weeks and then healed. As the result of this injury the fibrous inelastic bands of the cicatrix prevented the expansibility of the os uteri sufficiently for the birth of the child.

ART. 153.—*Defective Insertion of the Placenta.*

By Dr. JONNIA-REYMOND.

(*Gaz. des Hôp.* 85, 1865. *Schmidt's Jahrbücher*, No. 11, 1864.)

It has been usual to consider that a central placenta prævia was a more dangerous abnormality than a lateral one. Cazeaux, however, has repeatedly observed that, in central placenta prævia, hæmorrhage does not occur until the beginning of labour; and this accords completely with the doctrine advanced by him and by Jacquemier. According to this doctrine, the pregnant uterus, during the last three months, begins to undergo development at the cost of its lower segment; so that, if the placenta be seated in this segment, it is liable to be disturbed during the time specified, and to occasion frequent bleeding. At this time, so remote from the natural termination of pregnancy, the plug and rupture of the membranes are of little avail for the gradual dilatation of the os; and the hæmorrhage may be sufficient to destroy life before it is possible either to turn or extract the fœtus, or to effect delivery by forceps.

In central placenta prævia, on the other hand, since the cervix is only taken up into the expanding uterus during the last fortnight of pregnancy, hæmorrhage is not to be feared until then. At that time the os is readily dilatable, and the pressure of the foetal head exerts a powerful influence in controlling the flow of blood.

The author therefore concludes that the more lateral the position

of a placenta prævia, the less favourable must be the prognosis. In No. 102 of the *Gazette des Hôpitaux*, Chaillons publishes three cases in support of this opinion.

ART. 154.—*Experimental Researches into the Duration of Fœtal Life after the Death of the Mother.*

By Dr. BRESLAU.

(*Monatschr. f. Geburtsh.* Aug. 1864. *Schmidt's Jahrbücher*, No. 2, 1865.)

The author instituted a series of experiments in order to determine how long a fœtus would survive the death of its mother. The animals experimented upon were hares and guinea-pigs. It was found very difficult to determine when the mother was very near to the close of her gestation; and more difficult still to determine the precise moment of death in either mother or offspring. The author assumed death when the movements of the heart were reduced to a minimum, and when peripheral circulation had ceased, when respiration was completely stopped, and when no movements of the limbs, either voluntary, instinctive, or reflex, were produced by ordinary stimuli. Between evident death and evident life he recognizes an intermediate stage of *apparent death*, during which some one of the above-named functions may still be in some degree carried on. His conclusions were as follows:—

Where the mother has been suddenly killed:—

1. The life of the fœtus always continues for a time, after the maternal death.

2. The life of a fœtus contained within a dead mother is soon placed in extreme peril, which is shown by the violent and spasmodic movements. The character of these movements leads to the inference that they are excited by want of oxygen, and that they are premature respiratory efforts.

3. The fœtus commonly passes into the condition of apparent death by the end of the first minute after the death of the mother. Within the ovular membranes this apparent death may continue, under favourable circumstances, for eight minutes; but it usually passes into actual death much sooner than this.

4. An apparently dead fœtus removed from the mother is nearer to death than to life; and never recovers spontaneously.

5. Only in a few favourable cases was a living fœtus extracted at nearly five minutes after the death of the mother. After the lapse of three minutes, the probability of extracting a living fœtus is very small.

6. Later than five minutes, after the death of the mother, the fœtus is never found living, nor apparently dead later than eight minutes. After that time, actual death has taken place.

7. Death occurs earlier by some minutes in an unborn fœtus.

8. The kind of death suffered by the mother has some influence

on the duration of life of the offspring. The suffocation of the mother is unfavourable. Bleeding, death by chloroform, and by injury to the nervous centres, are more favourable.

9. The fœtus of the hare appears to be more independent of the maternal life than that of the guinea-pig.

10. It appears to be an important element in the duration of life, whether the fœtus be mature.

Applying these conclusions to the human species, and to questions of practice, the author says :—

1. It cannot be doubted that the human, like the brute, fœtus, will survive for a time the sudden and violent death of the mother.

2. Daily experience shows that the human fœtus possesses greater power of endurance than that of a small mammalian; and it may therefore be believed that the human fœtus would survive the maternal death for longer periods than those above mentioned.

3. It would, therefore, be the duty of the surgeon to perform the Cæsarean section as soon as possible after the manifest death of a woman advanced in pregnancy; unless the fœtus were known to be already dead, or unless a more rapid delivery could be effected *per vias naturales*.

4. Every exertion should be made to resuscitate an apparently dead fœtus, brought thus into the world.

5. The Cæsarean section would not be expected to yield a fœtus only apparently dead, unless it were performed within the first fifteen or twenty minutes after the death of the mother.

6. If the maternal death has been caused by any blood disease, such as cholera, typhus, scarlet fever, small-pox, or the like, there would be no prospect of removing a living fœtus. The same applies to death by rapid poisoning, which speedily affects the whole mass of the blood, and would destroy the offspring together with the parent. Death by chloroform appears to be an exception; since chloroform as such does not enter the fetal circulation.

(B) CONCERNING THE DISEASES OF WOMEN.

ART. 155.—*On Hospital Midwifery as a Cause of Puerperal Fever.*

Dr. ROBERT BARNES, Obstetric Physician to St. Thomas's Hospital, &c.

(*Lancet*, February 11 and 18, 1865.)

In an excellent lecture on the prevalence and causes of puerperal fever, Dr. Barnes makes the following remarks upon this particular cause. A comparison of the tables showing the mortality in the four lying-in hospitals in London and at the Royal Maternity Charity furnish the data for his conclusions :—

“During the ten years, 1853-62, the Royal Maternity Charity

delivered 84,026 women. Of these, 94 died from all causes, giving 1 death in 362, or 2·7 per 1000. 36 only of these deaths were due to puerperal fever, being 1 in 944, or 1·06 per mille. The proportion of deaths from puerperal fever in the Charity is, therefore, less than in the general population (hospitals included), both in proportion to the total labours and to the deaths from all causes.

"It is not easy to obtain full information as to the statistics of the lying-in hospitals of London. A direct challenge thrown down to these institutions by the *Lancet* in 1862 produced, however, statements from the Queen Charlotte's and the British lying-in hospitals. With regard to Queen Charlotte's Lying-in Hospital, Dr. George Brodie states that 'for the last thirty-six years—from 1828 to 1863 inclusive—the registers have been carefully kept; but no detailed accounts published. During this period, 7736 patients were delivered in the hospital.' Of these, 198 died. This gives a mortality of 1 in 38. There is no parallel to this in London domiciliary practice. To account for a result so disastrous, we are reminded that the hospital admits a large proportion of single women, who enter broken down by physical and mental distress. Thus 3611 were single women. Amongst these the mortality was 1 in 28, The mortality amongst the married women was 1 in 57. The difference is remarkable. But again, I must observe, that domiciliary practice in London offers no approach to this mortality amongst married women. The mortality in the Royal Maternity Charity, whose patients are all married women, but including a large number living in the most abject condition, is only 1 in 362.

"In Queen Charlotte's Hospital the death-rate from puerperal fever alone was 1·5 per cent. This, out of a total death-rate of 2·6 per cent., gives more than half the mortality to puerperal fever, a disease which in the domiciliary practice of the Royal Maternity Charity accounts for less than half of the deaths in childbed.

"Another hospital which responded to the challenge is the British. This is a very small institution, the annual deliveries not exceeding 120. During the thirteen years 1849-61 the death-rate was ·69. The patients were all married women. But we see that, even amongst these, when subjected to hospital influences, the mortality is out of all proportion to that observed in domiciliary practice.

"The General Lying-in Hospital has not, that I am aware of, published any authentic history of its proceedings; but it is well known that puerperal fever has on many occasions raged fatally within its walls, compelling the governors to shut up the institution in order to arrest the scourge. Dr. Ferguson based his admirable work on Puerperal Fever upon observations, terminating in 1838, made in this hospital. The late Dr. Rigby gave me a statistical statement of the practice of this hospital from 1833 to 1856. From 1856 to the present time I am only able to show the total deaths as published by the Registrar-General. From Dr. Rigby's statement it appears that the deliveries averaged 206 yearly. The total number of deliveries during the twenty-four years was 4950. The total deaths amounted to 146, giving as nearly as possible a death-

rate of 3 per cent. The greater part of this terrible mortality was certainly due to puerperal fever. During a portion of the period comprised in the foregoing statistics, the General Lying-in Hospital was under the influence of bad drainage; but this fact does not invalidate the general argument advanced.

"If we sum up the experience of these three lying-in hospitals, and contrast it with that of the Royal Maternity Charity, we shall possess a comparative view—not an unfair one—of the results of home and hospital midwifery. The observations apply to the same population, and very much to the same periods. 14,236 deliveries in the three lying-in hospitals gave 366 deaths. Thus we have a proportion of deaths in hospital practice to home practice of 25 to 3 in 1000.

"I do not think any other conclusions can be come to than that the hospital excess of 22 deaths in every 1000 lying-in women is due to hospital causes; and that the chief of these causes is the manufacture of puerperal fever.

"I have confined myself hitherto strictly to London practice—that is, to facts capable of easy verification by others. But in whatsoever direction similar inquiries be extended, we meet with similar results, and thus obtain so large and uniform confirmations of the conclusions I have stated that these become absolutely irresistible.

"But the system of hospital midwifery in this town and country is happily restricted within the narrowest limits. The hospitals are not only few, but are, as we have seen, extremely small. The evils attendant upon them must therefore be inconsiderable if compared with those seen in other countries where hospital midwifery is the rule rather than the exception. It is therefore a question of the last importance to keep clearly in view what are the fruits and consequences of this system when pursued upon a large scale. I now propose to consider the proofs that the puerperal fever of the lying-in hospitals is in reality the produce of the hospitals.

"1. In Germany especially great ingenuity has been expended in endeavouring to trace puerperal fever to that peculiar condition in lying-in women which we may call the puerperal constitution. This is derived from the changes effected in the system during pregnancy, by labour, and by the puerperal process. There is no doubt that the blood of the pregnant and puerperal woman, and her functions of secretion and excretion, are often profoundly modified: it must be freely admitted that these modifications do predispose her to febrile affections. The influence of this constitution cannot be overlooked. It goes, as I believe, far to account for a large proportion of the cases of puerperal fever belonging to our second great division—those, namely, which depend upon causes springing up within the patient herself. But, strictly speaking, it will account for no more. It will not by itself develop into puerperal fever on the scale and in the frequency with which we see this disease in hospitals. If it were alone sufficient, we ought to find puerperal fever as frequent in home midwifery as in hospital midwifery. The fact that puerperal fever is so far more frequent

in hospitals must then depend upon other conditions especially belonging to the hospitals.

"With this strictly logical deduction many minds will perhaps be satisfied. But other arguments connected with the question are so important as to demand careful consideration.

"2. It has been contended, in Germany and France, that in many cases the fever which rages in the hospitals is only a manifestation of an epidemic influence acting upon the population generally, outside as well as inside the hospitals.

"No doubt there are examples of the simultaneous prevalence of puerperal fever inside lying-in hospitals and amongst the population of the towns in which these are situated. This simultaneous prevalence may be presumed in some cases to depend upon a common cause; but certainly this is not necessarily the case, and it is still a question whether, properly speaking, we ought to admit the existence of such a thing as epidemic puerperal fever—that is, in the sense in which we speak of epidemic cholera or influenza. A rigorous investigation of individual cases would, I believe, certainly reveal the fact, that every case of so-called epidemic puerperal fever was the product of a poison conveyed to the patient from some definite source. If this be so, we might as reasonably talk of an epidemic vaccinia whenever a number of persons have been simultaneously vaccinated.

"But if we admit the fact of the occasional simultaneous prevalence of puerperal fever inside hospitals and outside, it still remains to be proved that in such cases the fever got into the hospital from without. In not a few instances it is quite certain that the fever proceeded in the converse direction—that is, from the hospital to the town. Indeed, it is commonly found that the first cases break out in the hospitals; and what is more conclusive still, it has frequently happened that there has been no puerperal fever anywhere but inside the hospitals.

"A comparison of the tables showing the mortality in the four lying-in hospitals of London and of the Royal Maternity Charity during the ten years 1853-62, will sufficiently prove this proposition. Whilst in the practice of the Charity the cases of fever were so rare that almost all might be attributed to causes generated in the patients themselves, or to the taking of prevalent epidemics, as typhoid or scarlatina, the hospital patients succumbed in such numbers that their deaths can only be ascribed to some special malignant influence."

ART. 156.—*On Puerperal Fever.*

By Dr. T. SNOW BECK.

(*Proceedings of Obstetrical Society of London*, Feb. 1, 1865. *Lancet*, April, 1865.)

The author relates minutely the case of a young lady aged twenty-four years, who was confined of her first child, presented symptoms of puerperal fever on the second day, and sank three days after—

wards, or on the fifth day after her confinement. He points out that the leading feature in the case was the absence of that contractile power of the uterus necessary to the expulsion of the child and to provide for the safety of the mother after its birth, this being apparent at each stage of the labour—the languid contractions of the uterus, the inability to increase the force of these contractions by the usual agents, the requirement for the use of the forceps, the necessity for the removal of the placenta, the hæmorrhage which followed, the existence of a large flabby open uterus when the hand was introduced, great difficulty to induce sufficient contraction of the organ to prevent further hæmorrhage, and the impossibility to procure a firm and consistent contraction after the cessation of the hæmorrhage. This lax condition of the uterus permitted the uterine sinuses to remain pervious, and arose from the absence of that due contraction of the muscular tissue which is essential to prevent any fluids circulating along their canals.

Another case of puerperal fever is given, which also occurred in a healthy young woman, after apparently a perfectly natural labour, and which proved fatal on the ninth day. The body was examined thirty-six hours after death, and copious effusion of fluid into the abdomen, with shreds of soft friable recent lymph, and some injection of the peritoneum, were found. The uterus was large, pear-shaped, even on the surface, soft, and rather flabby; its interior was everywhere covered with a soft membrane, which presented the usual microscopic characters of mucous membrane, and was covered by a red mucous secretion. All the tissues of the uterus were examined, and found to be perfectly healthy; no appearance of inflammatory product presented itself in any part of the organ. But the uterine sinuses were open, and water thrown into the larger veins of the abdomen readily traversed these canals, and escaped at the open orifices on the surface of the uterus.

From the careful examination of these cases the following deductions are drawn :—

1. The phenomena of puerperal fever may be produced by the introduction of poisonous fluids into the general system.

2. The uterine sinuses remaining pervious to the flow of fluids would afford a means by which the poisonous fluid or fluids would enter the system.

3. The pervious condition of the sinuses remained in consequence of the absence of that firm and persistent contraction of the uterus after childbirth which appears necessary to effectually close these canals, and prevent all circulation of fluid along them.

4. The secretion from the interior of the uterus was probably sufficient, when mixed with the blood, to induce the effects observed. And it would further follow that—

5. The various phenomena observed in puerperal fever may arise from this cause, modified infinitely by many incidental states; and the various inflammatory actions and products observed in the course of the disease would not be the essential parts of the disease, but morbid phenomena which occurred during the course of it.

6. The primary though not the only object in the prevention of

these attacks of puerperal fever will then be to procure a firm, complete, and persistent contraction of the uterus after the birth of the child, and thus effectually to shut off all circulation within the vessels of this organ.

The author considers that these cases are a decided negative to the opinions that puerperal fever is caused by uterine phlebitis, lymphangitis, endometritis, metritis, or any similar inflammatory condition of the uterine organs, as no product of inflammation was anywhere discovered after a careful and even microscopic examination. The results are also opposed to the opinion of Cruveilhier, who compared the internal surface to a vast solution of continuity, and the gaping orifices of the sinuses to the open-mouthed vessels of an amputated limb, for here the internal surface of the organ was everywhere covered by a soft membrane containing all the elements of the mucous membrane, and covered by a red mucous secretion; whilst the gaping sinuses could only be compared to the open-mouthed vessels of an amputated limb when the uterine sinuses were pervious and admitted fluids to circulate within them: the real point of comparison not being the open orifices, but the open canals leading from these into the veins of the general system. The comparison between puerperal fever and surgical fever is also founded on an erroneous basis; for again the natural condition of the vessels of an amputated limb was compared with an unnatural state of the vessels of the uterus, which ought not to be, and might in the majority of cases be prevented. The author differs essentially from Dr. Ferguson on the causes of the vitiation of the blood and on the varieties of puerperal fever.

The effect of this noxious impregnation of the general system is next considered, and it is shown that the quantity modified the result in a most singular way—a small quantity being eliminated by intestinal or urinary secretion, whilst a larger dose killed. When the uterus was very lax, and admitted of a ready flow of noxious fluid through the sinuses, the woman was stricken down as if by some fell pestilence, and sank in a few hours—"where the secretions are all suspended, and the patient sinks with rapidity." Where the deleterious fluid is introduced in smaller quantity, the system after a vain struggle with the poisonous infection, sinks in a few days, the chief morbid appearance after death being extensive peritonitis of a peculiar character, copious exudation of soft friable lymph, and much serosity. To a yet smaller amount of deleterious impregnation were attributed uterine phlebitis, metropéritonitis, distension of the lymphatics with purulent fluid; the effects being more local, and the inflammations being the consequences of the first changes induced. Whilst a still smaller amount of infection would produce low febrile conditions, extending over an indefinite period, and sometimes inducing purulent infiltration in various organs of the body. Each of these states being influenced by a variety of concomitant circumstances—as the original condition, state of health, the character of the fluids secreted, the existence of any diathetic disease, epidemic influences, &c.

On the question of contagion and the occurrence of epidemics the

author shows that the most experienced in this disease varied so much that no decided conclusion could be drawn. The prevention of the disease, it is urged, could be most effectually accomplished by procuring a complete and persistent contraction of the uterus after the completion of the labour; and that the means usually recommended were not sufficient for this object, it being generally considered sufficient to procure such an amount of contraction as to prevent any hæmorrhage, though it was necessary to go beyond this point of contraction before the safety of the woman could be secured. A nourishing and supporting diet was also necessary to remove the physical fatigue and mental anxiety of the labour, to restore the health from the waste occasioned by the previous months of pregnancy, and to enable the woman to pass through the subsequent changes which had yet to take place; much, however, depending upon the previous habits, state of health, and other incidental circumstances.

On the subject of treatment, when the disease was once developed, the principles are considered to be—(1) to prevent the further injurious impregnation of the system, either by obstructing the further flow along the uterine sinuses, or by removing the noxious fluids from the interior of the uterus; (2) by supporting the system during the struggle in which it is engaged, and by meeting any incidental complication which might present itself; and (3) perhaps a further source of treatment is now afforded, which might enable us to counteract, to some extent at least, the deleterious impregnation which has already taken place. The first would be attained by procuring, if possible, the further contraction of the uterus, or by inducing the coagulation of the blood in the uterine sinuses. But the principal curative means appear to rest upon the removal of all noxious fluids from the interior of the uterus, by cleansing it with a tepid solution of any sulphite or hyposulphite of soda each day or oftener; and should any fluid gain entrance into the uterine sinuses, it would probably be more beneficial than otherwise. The means to support the system are too well known to require further notice; whilst deleterious impregnation, which had already taken place, might be met by the internal administration of sulphite of magnesia or lime, in doses of one scruple to half a drachm every two or three hours.

ART. 157.—*On Dilatation v. Division of the Cervix Uteri.*

By Dr. GREAM, one of the Physicians to H. R. H.
the Princess of Wales.

(*Lancet*, April 8, 1865.)

The remarks which follow are in a letter to the editor of the *Lancet*, commenting upon some statements by Dr. Marion Sims on the subject of enlarging the os uteri by artificial means. Dr. Gream writes:—

"Had Dr. Marion Sims confined his remarks to eulogizing the plan of dividing the cervix uteri with a knife, his views, although differing from mine, would nevertheless, so far as I am concerned, have remained uncontested. But when he repudiates dilatation as dangerous in all its aspects, and declares that division of the cervix is as safe as dilatation is hazardous, it is due to the younger practitioners, whom he warns against acting too readily upon written evidence, that some one whose experience on the subject may be allowed to be somewhat extensive, should refer to the matter from a different point of view.

"It is now many years since this operation was proposed by certain practitioners as a cure for dysmenorrhœa and sterility; and from that time I have repeatedly been consulted by women who, at different dates antecedent to my seeing them, had undergone the operation; and, except in one solitary instance, I have met with no one in whom pregnancy had supervened upon this mode of treatment; indeed, their application to me was consequent upon the failure of the plan which had been adopted.

"The exceptional case was this. A lady to whom the birth of an heir was most important found herself with child, and sought my attendance when she had arrived at about the fourth month of pregnancy; telling me—without, however, attributing her pregnancy to the operation—that the uterus had six years before been operated upon; and so complete had been the division of the cervix that the finger could readily be introduced into the uterine cavity, and the membranes of the ovum could be touched, as they can be sometimes during the last days of gestation. The patient was an old friend of a physician whose name it is only necessary to mention to give assurance of his ability to judge of these matters; and I begged that his opinion might be asked in reference to the chance of a continuance of the pregnancy. His opinion corresponded with my own, and, according to our prognostications, abortion resulted but a few weeks afterwards, from the inability of the uterus to retain its contents. I need not say that the hopes of this patient are irreparably gone.

"I could relate cases of cellulitis, of pelvic abscess, and of chronic as well as of acute metritis, depending entirely upon the performance of this operation; but the following case will show some of its ill effects. A lady arrived in London in a deplorable condition, the effect of an operation upon her uterus which had some time before been performed. She was most desirous to reach home, and had been permitted to do so only upon her urgent entreaties, because her general condition was such as to give alarm lest she might not accomplish her journey. The physician who had been attending her, whose advice was sought after the operation had been performed, and when its bad effects became apparent, wrote to me, begging me to give her my most serious attention. It was desirable that she should be often seen; and Mr. Morgan, of Sussex-place, kindly undertook the charge of her, living as he did near to her own residence; and he and I conjointly attended her for months during the formation of an extensive pelvic abscess, which ultimately discharged itself

into the intestine, and she recovered, but with this result: her uterus is permanently fixed to the left side of the pelvic cavity, and any chance of future conception is totally at an end.

"I could relate cases of alarming hæmorrhage immediately following division of the cervix, and others in which the lamentable consequences were deferred, and in which death ensued, and I confess I was surprised to find that Dr. Marion Sims had been assured by some of the promoters of this plan of treatment that they had never met with hæmorrhage; for I fancied that my memory carried me back to evidence of a contrary nature. But of this I will say no more.

"The treatment of sterility is but seldom called for, except by those in the more wealthy classes of society, as the birth of children to persons amongst other classes is comparatively of less importance; and hence experience is gained in matured private practice rather than in hospitals. It seems incumbent on those, therefore, who have had the opportunity of treating cases of this kind, to make public their experience; and I am sure that those of your readers to whom I am known will give me credit for stating the truth, when I say that there are numbers of women in the upper classes of society who would sacrifice half their lives to become mothers, but who, from having undergone the operation of division of the cervix uteri, have lost all chance of being pregnant.

"I agree with Dr. Marion Sims that dilatation may prove hurtful, indeed may be dangerous to life; but not when properly performed. I have myself seen the uterus enlarged to the size of the fifth month of pregnancy by the undue means taken to dilate the os. I have known instruments so rudely forced through the canal that acute symptoms have followed, requiring most active means for their suppression. And I have myself found pieces of metal fixed in the uterus, which had remained there, in some instances, for days, and in others for longer periods; while inflammation was set up, and abscess resulted. I have known repeated dilatations followed by chronic as well as by acute inflammation; and this in single women. In one case—a case ultimately in charge of Mr. Turner, of Sussex-gardens, who asked me to meet him in consultation concerning it,—there had been no less than sixty instrumental introductions with the view to remove dysmenorrhœa; and this in an unmarried woman. But these are not the cases exemplifying mischief by dilatation; for common experience shows that the uterus will not admit of the permanent introduction of foreign bodies without bad results, and prolonged repetition of dilatation is in no case called for. I entirely discard, therefore, all such cases as arguments against dilatation, as well as others mentioned by Dr. Marion Sims; and I repudiate, with all the power that words possess, the tampering with the virgin uterus under any but the most urgent suffering.

"I remember when I was a pupil of Sir Benjamin Brodie, at St. George's Hospital, that great surgeon introduced the practice, when extracting a calculus from the female bladder, of dividing in two or three places the mucous membrane of the urethra, throughout the whole canal, by means of a bistouri caché, in order that, as cicatri-

zation proceeded in these wounds, the urethra might contract, and thus prevent the incontinence of urine which usually followed in such cases.

"Now it appears to me that there is no difference in the behaviour under cicatrization between the mucous membrane of the cervix uteri and that of the urethra. Hence this operation becomes rather an impediment to than a cause of pregnancy. And when it is remembered that we are acting upon the most dilatable part of the whole body—so dilatable, indeed, that the foetal head even escapes through it by dilatation—why should we seek other methods for enlarging the os uteri than dilatation, when it alone is the process especially pointed out as fitted to the nature of the part under treatment?"

"I regret exceedingly to differ from Dr. Marion Sims; but when I have been witness for many years past to the cure of dysmenorrhoea in married woman through dilatation of the os uteri, and when I have seen pregnancy repeatedly and often immediately supervene upon the same operation, it has occurred to me that I have some claim to your indulgence and that of your readers for considering the two questions of incision and dilatation in an opposite point of view to that entertained by Dr. Marion Sims.

"It is far from my wish to be supposed to suggest to so experienced a practitioner as Dr. Marion Sims appears to be the adoption of a mode of treatment which differs from his own; but he has evidently taken his objections to dilatation of the os uteri from cases which in my view were evidences, not of dilatation, but of maltreatment of the uterus. If, however, he will try the experiment of introducing a very small flexible metal bougie, such as is used for the male urethra, about two inches into the os uteri, and follow this by others gradually increased in size until the canal is dilated to the size of a common pencil; or if he will use a dilator such as was mentioned by Dr. Priestley, I think, in the *Lancet* a few months ago, or one which was made for myself, and which was described by me in your journal—he will find it more successful in cases of sterility than slitting the cervix uteri, and thus inflicting a permanent injury on that organ.

"The whole process of dilatation may be performed in two or three operations at the most."

ART. 158.—*On Cauterization with Nitrate of Silver in Pruritus Pudendi.*

By M. DIEUDONNÉ.

(*Journal of Practical Medicine and Surgery, and Glasgow Medical Journal*, April, 1865.)

Mr. Dieudonné informs us that cauterization with nitrate of silver is the only truly efficacious method of treatment in pruritus pudendi. Twenty years' experience have led him to the conclusion that one or

two superficial applications of lunar caustic promptly allay the irritation. "So convinced am I," says M. Dieudonné, "of the virtue of the remedy, that I do not hesitate to prognosticate a speedy and entire cure to those who will submit to this mode of treatment."

ART. 159.—*On Habitual Abortion in Flexion of the Uterus.*

By Dr. HÜTER.

(*Monatsschrift f. Geburtsk.* September, 1864; and *Schmidt's Jahrbücher*, No. 2, 1865.)

It is well known that many women are unable to carry an ovum to the full term of utero-gestation, and that they abort at the same period, in successive pregnancies, without manifest cause. This has been explained by supposing that the first abortion impresses upon the uterus some peculiar weakness or tendency to abort; so that the event recurs, even although the original cause be no longer in operation. The author does not accept this explanation, but believes that every abortion must have some determinate cause, and that the presumed "tendency" to abortion, as a mere result of previous abortions, exists only in imagination.

While, on the one hand, it may very well happen that the so-called habitual abortion, in any patient, may depend upon the continued action of the same cause, it may also depend, each time, upon a different one. It lies beyond the scope of a journal article to discuss all the possible causes of abortion; and the author confines his observations to three cases, which appeared to depend upon ante-flexion of the uterus. It is well known that abortion may be produced not only by flexions, but also by other malpositions of the uterus. This has been explained by supposing that the uterus, in its unnatural position, exerts compression upon both its own and the pelvic bloodvessels, and thus impedes the return of venous blood from its tissues. In this way is produced a permanent congestion of the uterine parenchyma; and this, at the period of menstruation, may lead to rupture of the vessels of the hyperæmic uterine placenta, or of the decidua. Such rupture may produce sufficient hæmorrhage to detach the ovum and produce its expulsion; or it may be only enough to threaten an abortion which treatment will prevent.

The ordinary chronic hyperæmia of uterine flexion, which affects the mucous membrane as well as the parenchyma, is much increased at the commencement of pregnancy. Every flexion also modifies the changes of position of the gravid uterus, which, through its increasing weight, becomes more or less ante- or retro-verted. Hence there is an addition to the ordinary impediment to the return of venous blood, and, at the same time, an unusually active flow of arterial blood. The increase of the ante- or retro-version is what chiefly requires attention; since this, and not the flexion, is the exciting cause of abortion.

Anteflexion is the malposition that most frequently causes

habitual abortion, although it may often not be recognized at the time, being much diminished by the contraction of the longitudinal muscular bands, so that the uterus may come to have its long axis coinciding with that of the pelvic entrance. As involution takes place, the former flexion reappears.

As practical rules, the author lays down the following:—1. If a woman suffering from flexion become pregnant, she should be examined in the second, or, at latest, in the third month of her pregnancy, in order to ascertain what changes of position the uterus has undergone. 2. In any case in which abortion has already commenced, without assignable cause, an examination should be made some time afterwards, in order to ascertain whether there be any flexion present, which, during the actual period of abortion, was for the time overcome by muscular action.

In mentioning the want of statistics upon the question of the connexion between flexion and abortion, the author points out that the power to produce an injurious degree of congestion must depend upon the degree of the malposition; and that the slighter grades may be entirely harmless.

The rational treatment consists in an endeavour to remedy the flexion in the non-pregnant state, so as both to facilitate conception, and to prevent the recurrence of the dangerous ante- or retro-version when pregnancy takes place. The absolute cure of a flexion can only very seldom be effected; but sufficient improvement for the end in view may generally be obtained. After such improvement, it is still necessary to examine the position of the uterus in early pregnancy, and to see if it can be modified with advantage.

The author's treatment consists in confining the patient to bed eight days before the time at which the second menstrual period would occur if she were not pregnant; and in a daily endeavour to rectify, by the fingers, any malposition that examination may detect. If the tendency be towards anteversion, the patient must lie on her back; if towards retroversion, on her side. She must remain in bed for eight days after the menstrual time, continuing the same daily treatment, and then may rise, and move about cautiously, avoiding strong exertion. Eight days before the third period she must return to bed, and again undergo daily manipulation; but, when the first week of the fourth month has passed, if the uterus (as is usual) has then risen well above the pelvic brim, all fear of ante- or retro-version may be laid aside, the patient may leave her bed, and return to her ordinary mode of life.

The author concludes by expressing his belief that flexions of the womb are not cured by ordinary pregnancy, but that they are reproduced after labour at full time.

ART. 160—*A Serious Complication of Retroflexion in the Uterus.*

By M. VELPEAU.

(*Journal of Practical Medicine and Surgery*, November, 1864.)

A case of retroflexion of the uterus, lately under his care in the Hôpital La Charité at Paris, caused M. Velpeau to make some clinical remarks on the complication in question. The case is this:—

CASE.—A woman, aged twenty-eight, suffering from dull, permanent pain in the abdomen. No fever was present, but the patient complained of deep-seated pain; she walked with much difficulty, and when she coughed or stumbled she experienced twinges and a sensation of dragging, which checked her further progress. Examination, *per vaginam*, betrayed the existence of retroflexion of the womb. This is by no means an unfrequent affection, but it gives rise to extremely variable symptoms, and the prognosis cannot, therefore, be the same in all cases. M. Velpeau remarked that in very many women the disease gives rise to little, if any, derangement of health; whereas in another equally large series of subjects inflexion of the womb induces a variety of morbid manifestations, amongst which may be noted erratic pains in the abdomen and back, and sometimes merely an uneasy sensation in the inferior extremities. In some women, however, the entire nervous system is disturbed by the uterine deviation, or the patients suffer from obstinate constipation, a sense of weight in the rectum, or severe dysmenorrhœa. The disease, in addition, occasionally becomes an unquerable impediment to impregnation.

M. Velpeau invited attention to the fact that in this woman the pelvis was filled by a hard, irregular, lardaceous tumour formed by the uterus and its appendages. From this circumstance it might be inferred that the retroflexion had coincided with chronic suppurative inflammation of the peri-uterine cellular structures, puriform matter having been found in the motions, doubtless supplied by an abscess which had opened into the rectum.

"This complication," says M. Velpeau, "is not of frequent occurrence; indeed, retroflexion is, in general, a mere deformity often coincident with an otherwise perfectly healthy condition of the womb. If the fundus, however, presses upon the adjacent organs, a certain amount of mechanical irritation may be the consequence; and during defecation, coughing, or retching, the viscera, impelled downwards by the action of the diaphragm, weigh upon the fundus uteri, and thus become an active cause of congestion and inflammation of the peri-uterine cellular structures. Such a condition is undoubtedly serious, and must induce the practitioner to pause in forming his prognosis. In this region, abscesses of the cellular tissue are not in communication with each other, but lie disseminated around the sacral or lumbar plexus, the broad ligaments, &c. These puriform collections, however, sometimes open into the rectum, and the patients recover after having for a time suffered from diarrhœa and hectic fever; but many perils still threaten the life of women afflicted with

this form of disease—a fact abundantly evident even on superficial inspection in the case under consideration. Organs previously unconnected are now attached to each other by morbid adhesions; viscera hitherto free to expand or contract without impediment, are now firmly bound in a lardaceous shell which interferes with every movement; hence, a more or less considerable disturbance of the functions of menstruation, micturition, and defecation. Pregnancy, under these circumstances, is an event much to be apprehended, distension of the womb necessarily involving a certain amount of laceration and pelvic inflammation, which, although it may not actually destroy life, must inevitably bring on miscarriage.

"The patient under consideration is, therefore, in a highly perilous position, although, in point of fact, the pains she complained of have yielded after a few days' rest, and although she might, by a superficial observer, be deemed to be labouring under uterine symptoms of very inconsiderable importance."

ART. 161.—*Statistics of 150 Cases of Ovariectomy.*

By Dr. E. R. PEASLEE.

(*American Journal of Medical Sciences*, January, 1865.)

These cases have all been *published* during the four years 1860, 1861, 1862, 1863; and they therefore form a sequel to Mr. Clay's statistics of 425 completed operations, published as an appendix to Kiwisch's Treatise on Disease of the Ovaries, which latter statistics included all the cases published up to 1860. The most important points of this long essay are summed up in the following analysis:—

"Of the preceding 150 cases of ovariectomy, 99, or 66 per cent. recovered; and 51, or 34 per cent. died. Of Mr. Clay's 425 cases, 57 per. cent. recovered, and 43 per cent. terminated fatally. I attribute the greater success of the operation during the last four years to recent improvements in the operation itself, and a more judicious after-treatment; both of these advantages having been secured in the cases adduced, since a large proportion of the operations were performed by experienced ovariectomists. I have, however, elsewhere shown that if we select only the recent cases of experienced operators *alone*, we find that over 82 per cent. have been saved by the operation.

"I have endeavoured, so far as possible, to deduce from the preceding data, the circumstances which mainly determine the result of ovariectomy; and will consider them under the four following heads:—

1. The condition of the patient when operated upon.
2. The manner of performing the operation.
3. The after symptoms.
4. The after treatment.

I will, however, premise an analysis of the

"*Causes of Death after Ovariectomy.*—The causes of death in the 51 fatal cases of ovariectomy, were as follows:—

"Peritonitis	12 cases	23 $\frac{27}{51}$ per cent.
Septicæmia (pyæmia)	9 "	17 $\frac{31}{51}$ "
Shock or collapse	7 "	13 $\frac{37}{51}$ "
Exhaustion	7 "	13 $\frac{37}{51}$ "
Hæmorrhage	1 "	nearly 2 "
Strangulation of intestine in incision .	1 "	" " "
Diarrhœa	1 "	" " "
Erysipelas	1 "	" " "
Tetanus	1 "	" " "
Ulceration through the bladder . . .	1 "	" " "
Cause not stated	10 "	" 19·6 "

"It is an interesting fact that only 1 out of 51 deaths was attributed to hæmorrhage. It is, however, not improbable that some of the deaths attributed to exhaustion were the final result of an oozing of blood too gradual to produce its effects at once. Septicæmia also, probably, in some instances, was produced by the decomposition in the peritoneal cavity of blood thus effused. It would probably not be an extravagant statement if we regard 5 per cent. of the deaths after ovariectomy, as being the result of hæmorrhage, either directly or indirectly. From the above analysis it appears that peritonitis, septicæmia, shock, exhaustion, and hæmorrhage, together destroy over 70 per cent. of all who die after ovariectomy.

"I. THE CONDITION OF THE PATIENT AT THE TIME OF THE OPERATION.—The circumstances included under this head are: Her age; the married or unmarried state; the general health; the kind, size, and duration of the tumour; the existence or not of adhesions, and of ascites; and the number of times tapped.

"1. *Influence of Age.*—The following table gives the ages in 116 of the cases here collated, and the results of the operation.

"Under 20 years	8 cases.	50 per cent. recovered.
20 to 25 "	16 "	75 " "
25 " 30 "	13 "	76·92 " "
30 " 35 "	24 "	45·83 " "
35 " 40 "	20 "	80 " "
40 " 45 "	10 "	60 " "
45 " 50 "	7 "	57·14 " "
50 " 55 "	10 "	80 " "
55 + "	8 "	85 " "
not stated	34 "	" "

"Thus the most unfavourable age is under 20; next, from 30 to 35; and the average of the whole decade from 40 to 50 is but 58·82 per cent. of recoveries. From 30 to 35 is perhaps the period in which the effects of child-bearing are most exhaustingly felt, while that from 40—and especially from 45—to 50, is another critical season for women.

"On the contrary, the highest percentage of recoveries occur in

those above 55 years (85 per cent.); and the average above the age of 50 is $83\frac{1}{2}$ per cent. The average from 20 to 30 years is 75·85 per cent. The oldest patient on whom ovariotomy has yet been performed is Dr. Bennett's, of Danbury, Conn., her age being 75 years. She recovered.

"2. *Married or unmarried State*.—Of 116 cases in the preceding list, 64 were married, 52 were single.

"Of the married, 38 or $59\frac{3}{5}$ per cent. recovered.

"single, 30 „ $73\frac{1}{5}$ „ „

"3. *General Health at Time of Operation*.—Of 98 cases the general health was robust in 41 cases, impaired in 47 cases, and broken down in 10 cases.

"Of those in robust health, 31 or 75·6 per cent. recovered.

" „ impaired „ 30 „ 63·83 „ „

" „ broken „ 4 „ 40 „ „

"This is a point of great practical moment, and should be noted in all future reports. 52 cases out of the 150 are not represented in the above specifications at all; though they included 34 recoveries and 18 deaths. It is my conviction that the most favourable condition under this head is *slightly impaired* health.

4. *The kind of Tumour*.—61 of the preceding cases were polycystic tumours, and 11 cases were monocystic.

"Of the polycystic, 42 or 68·85 per cent. recovered.

"monocystic, 8 „ 72·72 „ „

"Here again we need to include all the cases; since, as just shown, the recoveries in *both* kinds of tumours are above the general average of 66 per cent. of all operated upon.

"5. *Size of the Tumour*.—If we arrange the tumours weighing less than 15 lbs., under the head of "small," and the rest of "large," we find that—

"Of 6 cases of small tumour, 5 or $83\frac{1}{3}$ per cent. recovered.

" „ 49 „ large „ 37 „ 86·04 „ „

"The figures are here too high, as under the preceding head.

"6. *Duration of Tumour after it became appreciable*.—

"Of 23 cases of tumour of less

than $1\frac{1}{2}$ year 11 or 47·82 per cent. recovered.

Of 41 cases of tumour $1\frac{1}{2}$ year

and over 31 „ 72·72 „ „

"A longer duration generally implies a greater size, a considerable distension of the abdominal parietes, and some impairment of health—all of which I consider favourable circumstances.

"7. *The Existence or Non-Existence of Adhesions and Ascites*.—

"Of 41 cases of extensive adhesions, 25 or 60·97 per cent. recovered.

" „ 10 „ slight „ 7 „ 70 „ „

" „ 16 „ no adhesions, 14 „ $87\frac{1}{2}$ „ „

"Still, I adopt the opinion of Dr. W. L. Atlee, that unless the adhesions are visceral or pelvic, they do not essentially increase the danger of the operation, if performed by an experienced operator.

"Only 5 cases are noted of ascites as a complication, and of these 2 recovered, and 3 died. Very likely it did co-exist, however, in some of the cases in which tapping of the tumour was resorted to.

"8. *Effects of previous Tappings on Ovariectomy.*—The following table shows the result of ovariectomy in 57 patients who had previously been tapped from 1 to 12 times.

	Once tapped.	7 times tapped.	Three times.	Four times.	Five times.	Six times.	Eight times.	Nine times.	Twelve times.
Recovered.	12	11	2	1	2	4	2	2	1
Died . . .	2	10	2	1	2	2	1	0	0

"Of the preceding 57 cases, 37 or 64·9 per cent. recovered. This is but 1·1 per cent. below the general average. The recoveries of those who had been but once tapped, amounted to 85·71 per cent. even. I have always regarded a single tapping as favourable on the whole, since it generally implies health slightly impaired, and the other advantages mentioned under a preceding head (No. 6). Repeated tappings, on the contrary, imply much exhaustion in most cases; though not in the three above mentioned as 9 times, and 12 times tapped.

"II. THE MANNER IN WHICH THE OPERATION IS PERFORMED.—I shall consider, under this head, only the manner in which the pedicle of the tumour was managed, and the question whether, before closing the incision, the peritoneal cavity was sponged out, and whether the peritoneum was included by the sutures closing it.

"1. *Management of the Pedicle.*—I here distinguish two classes of cases.

"A. Those in which the *pedicle was left in situ*—whether after applying the double ligature (Dr. C. Clay's method), and which is brought through the lower end of the incision; or, after the ligature is cut close (Dr. T. Smith's method).

"B. Those in which the *pedicle is kept projecting externally through the incision*, being maintained in that position either by sutures, needles, or the clamp.

"Of 14 cases of the 1st class, 9 or 64·28 per cent. recovered.

"107 " " 2nd " 75 " 70·09 " "

"It may, however, be added that the cases in which the clamp was applied were, most of them, operated upon by experienced operators, especially by I. B. Brown and T. S. Wells. Recent facts lead me to conclude, however, that Dr. T. Smith's method will prove to be the best.

"2. *Was the Peritoneal Cavity sponged out before closing the Incision?*—I have not included the data for answering this question in my table; but I find (omitting all the cases in which there was no fluid to be removed from the peritoneal cavity) that—

"Of 50 cases in which it was carefully removed 35 or 70 per cent. recovered.
 „ 18 „ „ not removed, 10 „ 55½ „ „

"3. The data for answering the inquiry *whether the peritoneum was included by the sutures which closed the incision*, are also here omitted. I found that—

"Of 38 cases in which the sutures or needles were passed through the peritoneum, 27 or 71·05 per cent. recovered.

"Of 31 cases in which the peritoneum was *not* included, . . . 23 „ 74·19 „ „

"The causes of death, in the 11 fatal cases out of the 38, were not such as to be referred to the penetration of the peritoneum; and I think T. S. Wells' reasons for including that membrane quite conclusive.

"III. THE AFTER SYMPTOMS.—Many of the successful cases will be noticed as not having presented a single bad symptom after the operation. It cannot, however, be inferred from the non-appearance of bad symptoms during the first two or three days that the case will recover. The probability of peritonitis is thus diminished, but not necessarily that of septicæmia or of exhaustion.

"IV. THE AFTER TREATMENT.—The data on this subject are also omitted in the present statistics. I found the custom of giving powerful doses of opiates after the operation, on the decline in the last half of the quadrennial period; and my conclusion on that point is, *give just opiates enough to allay pain, as it may rise, and to secure sleep, and no more.*"

ART. 162.—*A New Method of Securing the Pedicle in Ovariectomy.*

By I. BAKER BROWN.

(*Proceedings of Obstetric Society of London*, February 1, 1865; and *Medical Times and Gazette*, March 25, 1865.)

The author observes that hitherto there have been three distinct methods of securing the pedicle; first, by ligature, allowing the ends to hang out, as practised by Dr. Clay, of Manchester (the pioneer of ovariectomy in this country, who has steadily led us on to our present successful results), and by Lane (the first surgeon in London who performed this operation) consecutively; secondly, by clamp, as first suggested by Hutchinson, and followed by many others; thirdly, by cutting off the ligature short and closing the wound, as first successfully practised by Rogers, of New York, in 1829, by Dr. Bellinger, in America, in 1835, by Dr. Siebold, of Darmstadt, in 1846, and recently by Dr. Tyler Smith. The first three gentlemen's cases were all successful, and the last-named gentleman has also had great success. Mr. Brown says that his objections to the first method had been the length of time required for

the ligature to come away, which varied from nine days in his own practice to a month in that of others; to the second, the frequent severe pain caused by the dragging of the pedicle, or the pressure of the clamp itself; to the third, the unsuccessful results in his hands following its use. Having repeatedly used the actual cautery of late, employing Dr. Clay's instruments, in burning adhesions off the omentum and elsewhere, he has gradually been led to the conclusion that the actual cautery might be employed in treating the pedicle itself. Consequently, on December 28, 1864, he tried it upon a patient of Dr. Burchell, of the Kingsland-road, a lady, forty-seven years of age, who had had three children, the youngest twenty-one years since. The disease was first discovered by Dr. Burchell in August last, and so rapidly increased as to lead Dr. Barnes and Dr. Tanner to recommend extirpation some short time before he (Mr. Brown) saw her. As the abdomen then was very large, the skin shiny, and the general health rapidly suffering, he performed the operation by Clay's large incision. There were many adhesions laterally and posteriorly, the bleeding from which was checked by the actual cautery; and finally, the pedicle, being secured by a clamp, whilst a very large multilocular mass of cysts was removed, was thoroughly seared by actual cautery and allowed to drop. The wound was then closed in the usual way, and it healed in a week, the patient being convalescent in a fortnight. Mr. Brown thinks that if this plan is found by repetition to be successful, it would very materially lessen the dangers of the operation, and consequently ensure a greater number of recoveries.

ART. 163.—*A Suggestion for the Treatment of Ovarian Tumours by Compression and Obliteration of the Tumour at its Base or Pedicle.*

By Dr. BENJAMIN W. RICHARDSON, Senior Physician to the
Royal Infirmary for Diseases of the Chest.

(*British Medical Journal*, March, 1864.)

The suggestion is simply to operate so as to compress and obliterate the tumour at its base or pedicle, either by ligature or acupressure, and thus to cut off its vascular connexion with the body; then to evacuate the fluid in the cyst, as far as is possible, by the trocar; and, lastly, to leave the cyst in the body to undergo natural shrinking and absorption.

The suggestion is based on the consideration, that an ovarian tumour is, after all, virtually an enormous aneurism. True, it is filled only with the water of blood, a little albumen, and a little saline matter; but all the fluid is derived from blood; and when death occurs, it is as from slow hæmorrhage. To cut off, therefore, the blood-supply from the tumour, would be to prevent the secretion of new fluid, and to stop the nutrition of the sac altogether.

From the comparative ease with which the ovariologist turns out the sac, when the abdominal walls are laid open, Dr. Richardson

cannot assume that the cyst derives any important blood-supply, except from its base; from the point, that is to say, where it originally was developed. If this be the anatomical fact, it follows that the nutrition of the cyst can be commanded at the base; and that to tie or otherwise compress the cyst there, and cut off all vascular communication from it, is simply equivalent to the performance of Hunter's operation on the femoral artery for the treatment of aneurism in the popliteal space, and is the same as removing the cyst itself.

The details of the operation, subject to modification, would be the following:—

1. The patient being under chloroform, a trocar should be passed into the cyst; the trocar should be so constructed that, without the necessity of removing it, the current from the tumour could be stopped at any moment, as the operator should direct.

2. When the body is relaxed to a proper extent by the withdrawal of fluid, a small incision should be made over the base of the tumour, and the parts dissected down until the tumour is reached. An incision such as is made for tying the common iliac artery would probably suffice.

3. The tumour reached, the operator would isolate its neck as low as possible, with the finger, and would then cast two strong ligatures, an inch apart, round the neck, with a large aneurismal needle. He might now entirely evacuate the tumour of its fluid contents, through the trocar, and then tie his ligatures; or he might tie first, and draw off the fluid afterwards.

4. The ligatures, cut off close, might be let remain in the abdomen; and, the wound being closed, and pressure being applied to the abdomen, the cyst may be left.

"I have here suggested" (says Dr. Richardson) "the compression of the neck of the cyst by a ligature, to apply which requires an incision. But in so doing, I only insist on the act of compression, not necessarily on the incision. I see indeed, if the principle be correct, that the details may be much simplified. It would not be difficult—for example—to pass through a very small incision, a long acupressure needle behind the tumour, and by a figure-of-eight twist round the extremities of the needle outside the abdomen, to bring the neck of the cyst fairly up to the abdominal wall and secure its compression.

"Or it might be possible to obliterate, subcutaneously, by means of a needle and thread only; I mean by passing a long curved blunt-pointed needle, armed with a strong thread, and introduced into the abdominal cavity by a subcutaneous incision, clean round the tumour at its base, and by tying the thread, after the needle was withdrawn, in a firm slip noose that should grasp the pedicle of the cyst with the required force for compression.

"Again, a clamp might be invented to open round the neck of the cyst, like the blades of a lithotrite, and to close by a screw movement upon the neck, and destroy the vascular connexion.

"If the principle thus suggested be sound, it will admit of application in all cases of ovarian tumours demanding operation. But it

has the advantage of being applicable in cases where the present operation is impossible; I mean in cases of multilocular cyst, or where the cyst is fixed too firmly by adhesions. It might be best to try the operation by compression in one of these cases first; in a case where, the present operation being hopeless, the patient must die, unless some other operation be at hand to save."

ART. 164.—*Spontaneous Rupture of the Uterus terminating in Recovery.*

. By Dr. BAUMANN.

(*Wurtemb. Corr. Bl.* xxxiv. 3, 1864. *Schmidt's Jahrbücher*, No. 12, 1864.)

The author found a woman in her fourth pregnancy with presentation of the left elbow and shoulder, and with some swelling of the right side of her abdomen, which was acutely painful to the touch. On introducing the hand to turn, it passed easily into the abdominal cavity; and, after extraction of the child, there was found on the right side a longitudinal rent in the uterus, four inches in length. A good deal of small intestine had passed through this rent, but was immediately replaced. The vagina was filled with sponge. The uterus contracted firmly, and the patient had comparatively little pain. Under the use of cold fomentations, large doses of morphia, lukewarm enemata, and subsequently compresses soaked in hot wine, she passed well through the first days. On the fifth day vomiting occurred, but ceased on withdrawal of the morphia; and on the sixth, after repeated enemata, abundant feculent stools were obtained, with much comfort to the patient, and much reduction of the previously tympanitic abdomen. In six weeks the patient was able to return to her ordinary occupation.

ART. 165.—*Case of Congenital Occlusion of the Left Half of the Vagina with Double Uterus.*

By Professor Dr. G. SIMON, of Rostock.

(*Monatschr. für Geburtsk.* Oct. 1864. *Schmidt's Jahrbücher*, No. 1, 1865.)

The author relates the case of a girl, fifteen years old, who was admitted into hospital for a tumour of the genitals. She commenced to menstruate at fourteen, and the discharge had returned three times; but she had, notwithstanding, suffered from symptoms such as are produced by mechanical retention of the menstrual fluid. Eight weeks after the appearance of the tumour she had profuse menorrhagia; by which, and by pain and discomfort, she was much worn down and exhausted.

On examination there was found, at the inner side of the left nympha, the rounded end of a tumour lying parallel to the vagina, and extending its whole length. It was cylindrical, fluctuating, and free from pain. On the right side of the tumour the finger passed

readily into the vagina; and, by using chloroform to relax the abdominal walls, the uterus could be readily reached, and examined simultaneously from the vagina and from the abdomen. An incision was made into the outer extremity of the tumour, and six ounces inspissated fluid blood escaped. Through this incision the finger passed into a second vagina, and found a second uterus at its summit. The two vaginae were completely separated by dense membrane. The wound healed rapidly, leaving a sufficient opening for the menstruation of the left-hand uterus; and the sufferings of the patient were entirely relieved by the operation.

The case is the fourth of the kind on record; and the first in which the condition was discovered during life, and relieved by an operation. The other three cases are related by Rokitsansky, Décès, and Beronius.

ART. 166.—*Congenital Absence of the Uterus.*

By Dr. FREUND.

(*Berl. Klin. Wchnschr.* I. 12, 1864.)

CASE.—The patient was a vigorous woman, thirty-eight years old, of feminine aspect, who had lived ten years in sterile marriage. In her nineteenth year she suffered from congestive headache, disturbed vision, difficult breathing, palpitation, and dyspepsia; and these evils were wont to increase every four weeks. In her twentieth year she had a mucosanguineous discharge from the genitals, that lasted for a few hours only, and reappeared once after an interval of six months. The first attempts at sexual intercourse were extremely painful, difficult, and attended by some bleeding and by escape of urine. After a time intercourse was completed without pain.

The external genital organs presented a natural appearance, but there was no vaginal opening. The urethral orifice gaped open to the size of a goose quill, and was much folded at its margin. The index finger passed readily along the urethra, encountered slight resistance at the sphincter vesicae, and passed into the bladder. A small Ferguson's speculum was also passed into the bladder without pain or difficulty, and it became evident that coitus was effected by the same channel. From the bladder no trace of any uterus could be felt, but by examining from the rectum and bladder together, and from the rectum and surface of the abdomen, there was found a small cylindrical body, about an inch high, in the folds of the broad ligament. The presence of ovaries was inferred from the menstrual effort and the feminine habit.

ART. 167.—*Sarcoma of the External Genital Organs, proceeding from the Periosteum of the Right Ischial Tuberosity.*

By Dr. SAEXINGER.

(*Prag. Med. Wchnschr.*, 1864. *Schmidt's Jahrbücher*, No. 11, 1864.)

CASE.—A peasant girl, twenty-two years old, had suffered for a month from pain about the anus and difficulty of walking, and discovered a hard swelling, as large as a hen's egg, between the anus and vulva. This swell-

ing was found, on examination, beneath the right labium, which it elevated. It was hard, of smooth surface, circumscribed, and painful to the touch. The skin covering it was freely movable. On separating the labia, the vaginal orifice was found covered by a round tumour the size of a small walnut, springing by a broad base from the inner surface of the right nymphæ. This tumour was of a pale, yellowish colour, with a dirty, brown scab at its most prominent point, from which proceeded a foetid ichor. It was intimately connected with the external swelling, which, again, could be traced back to the right ischial tuberosity. The inguinal glands were not enlarged. The patient suffered much pain in walking and sitting, and had entirely lost her appetite.

In the course of sixteen weeks the tumour had grown to the size of a man's fist, and filled the room with its foetid odour. The patient eat scarcely anything, slept badly on account of pain, and lost flesh rapidly. In consequence of a free bleeding, the tumour was removed by an *écraseur*. It was homogeneous, pale yellow, of fibrous structure, and moderate hardness. The microscope showed a delicate connecting tissue, with areolar arrangement, and in the meshes roundish cells, containing free granular matter or nucleoli.

Free suppuration, which continued for five weeks after the operation, effected the destruction of the deeper part of the swelling, and left a sinus. This afterwards closed, and in six months from its first appearance, there was no trace of the disease remaining.

ART. 168.—*Chronic Osteo-malacia with complete Re-ossification of the Softened Pelvis.*

By Dr. WINCKEL.

(*Monatschr. für Geburtsk. Mai, 1864. Schmidt's Jahrbücher, No. 11, 1864.*)

CASE.—Frau S. had given birth easily to four children; but in 1856, on account of osteo-malacia of the pelvis, was delivered of a dead child with great difficulty. In 1860 she was delivered of a living boy by the Cæsarian section. Early in June, 1863, she again became pregnant, and went on well until December, when she was suddenly seized with uterine pains and died almost immediately. The autopsy discovered that the uterus had ruptured at the cicatrix, and the foetus in its membranes had passed into the abdominal cavity.

The measurements of the pelvis taken after death, coincided almost entirely with those made prior to the Cæsarian section in 1860. The character of the deformity was that which would be produced, in osteo-malacia, by months of recumbency upon the right side; the right half of the pelvis being much narrower and more distorted than the left. The transverse diameter at the brim was four inches and one-third; at the widest part of the cavity, four inches; at the narrowest part of the cavity, three inches and a quarter; and at the outlet, two inches and three quarters. There was nowhere any trace of softening or flexibility, and the bones had not only recovered their proper density, but were harder, thicker, and stronger than those of a normal pelvis. The ilia were so thick as to be scarcely at all translucent in their thinnest parts; and their external surfaces were the seat of a considerable formation of new bone. The sacro-iliac synchondroses were partially ossified, in patches as much as an inch in diameter.

Complete recovery from osteo-malacia, such as is described above, is so rare, that its occurrence has been denied by many writers.

Litzmann, who, in eighty-five cases, noted sixteen of improvement or recovery, could only say that in these a curative process was going on. The author believes the above to be the first case of complete recovery that has been recorded.

ART. 169.—*New Sea-Tangle Tent, with Observations.*

By Dr. ROBERT GREENHALGH,
Physician-Accoucheur to St. Bartholomew's Hospital, &c.
(*Lancet*, April 8, 1865.)

After some introductory remarks upon the insufficiency of sponge tents, and upon the worthlessness of gentian-root tents, Dr. Greenhalgh says:—

"To Dr. Sloan, of Ayr, is due the credit of having first directed the attention of the profession to the dried stem of sea-tangle (*Laminaria digitata*) as a substitute for tents in ordinary use, and it is upon that substance I now wish to offer a few practical suggestions. It possesses the following advantages:—It is extremely abundant, cheap, can be easily worked, is cleanly, and possesses considerable dilating powers; its chief disadvantages being—its extreme hardness, the slowness of the dilatation, its imperfect absorption of those secretions which are prevented escaping from the uterus, and the difficulty of introduction by any instrument hitherto devised. Through the centre of a piece of laminaria, two inches and a quarter to two inches and a half in length, and of convenient thickness, a hole is bored longitudinally. Thus a tube is formed, giving peculiar advantages. At one end the tent is rounded off for facility and safety of introduction; at the other a small hole is pierced, about a quarter of an inch from its extremity, through which a loop of Chinese silk is passed, and secured by a knot. Thus the disadvantages formerly experienced are overcome. By means of the canal the introduction is greatly facilitated, for into its lower segment a somewhat flexible and gradually tapering metallic stem can be inserted. Its rapidity of expansion is also greatly increased, for the warm water in which it may be placed for some time prior to its use, now coming in contact with both its external and internal surfaces, renders it less hard for introduction, while it favours its more rapid expansion. Through the canal the secretions of the uterus can readily escape, for the hole increases in diameter proportionately with the expansion of its wall, which is in no way interfered with by the string attached to the side. Nor is the string liable to burst by the tension caused by increase in the size of the wall, for the enclosing loop may be of any size, or even the two ends need only be tied together. The length and diameter of these tents must be varied according to the size of the uterus and the amount of dilatation required. A very little practice will be sufficient to convince the most sceptical that the laminaria, used as I have suggested, possesses all the advantages without one of the disadvantages of the ordinary

sponge-tents. Moreover, thus prepared, it may be retained in the cervix uteri for two or more days, and its central opening, which dilates with the expansion of the substance of the tent, I have used as a means of introducing various substances into the body of the uterus. Upon this subject, however, I must defer any observations until my results have been tested by larger experience.

"In cases where the uterus is more or less sensitive, and dilatation is needed, I have found the hot hip-bath, with copious warm water vaginal injections, either with or without some sedative, not only favour more rapid expansion of the tent, but greatly relax the resisting tissues, and allay irritation. Occasionally I have prescribed an anodyne, or used sedative vaginal or rectal suppositories for the same purpose. In event of the pain becoming very acute, and especially if accompanied by vomiting or febrile disturbance, I have usually recommended the patient to send for me, or withdraw the tent herself. If the patient be of a very sensitive nervous system, especially if prone to hysteria, I generally prefer to leave the extraction to her own discretion, apprizing her beforehand that she will be sure to suffer pain, but to bear it if possible. By such a course I almost invariably find that the tent has been retained, and it has never yet been my misfortune to see any evil result.

"In cases of obliquity, I generally first introduce a modified Thompson's urethral dilator into the uterus, replace that viscus, and dilate to a greater or lesser extent, which facilitates the passage of the tents.

"One word with reference to the dilatability of the laminaria. I have in my possession specimens by which I have dilated the internal and external os uteri sufficiently to enable me to introduce my finger into the body of the uterus, and I have been informed by an American and an Australian, that in their countries specimens of the stalk of the sea-tangle are to be met with sufficiently large to dilate even an ordinary-sized vagina. If such prove to be the case, I can see no limit to the value of this agent as a dilating power. The tents above described may be procured of Messrs. Weiss and Son."

REVIEWS, BIBLIOGRAPHICAL NOTICES,

ETC.

- I.—*The Sanitary Commission of the United States' Army: a Succinct Narrative of its Works and Purposes.* 8vo, pp. 318. New York. 1864.

Military, Medical and Surgical Essays prepared for the United States' Sanitary Commission. Edited by WILLIAM A. HAMMOND, M.D., Surgeon-General U.S. Army, &c. 8vo, pp. 552. Philadelphia. 1864.

THE brightest portion of the history of the United States' civil war, now happily ended, is that which relates to the Federal Sanitary Commission. This commission in its inception, organization and operations is unquestionably the most remarkable feature which has characterized any war, ancient or modern. At the outset of the struggle, when, at the call of the Government, vast numbers of volunteers were enrolled and gathered together, it quickly became obvious to a few thoughtful minds that, unless some great organized effort were made for the sanitary welfare of the raw levies, a disaster as great as that which decimated the English army in the Crimea during the winter of 1855-6, must soon overtake the Federal forces. The resources of the Army Medical Department and the regulations of the War Office, devised for a small body of men and for a time of peace, were clearly insufficient to meet the requirements of large armies of raw soldiers amidst the exigencies of a campaign. It might well be doubted, however, whether the routine of the regular service was at all fitted to cope with the immediate needs of the new forces which had sprung into existence, and which must constitute the great bulk of the troops in the field. Revised departmental rules conceived for a state of things which no longer existed, would, most probably, not admit of adaptation to the new state of things which had suddenly sprung into existence.

The progress of events early showed that these surmises were correct, and on the 18th of May, 1861, a letter was addressed to the Secretary of War by the Rev. H. D. Bellows, Dr. W. R. Van Buren, Dr. Elisha Harris, and Dr. J. Harsen, representing three associations in the city of New York, namely, the Women's Central Association of Relief for the Sick and Wounded of the

Army, the Advisory Committee of the Board of Physicians and Surgeons of the Hospitals of New York, and the New York Medical Association for furnishing Hospital Supplies in aid of the Army, in behalf of the objects committed to them as a mixed delegation. The letter set forth that the three associations were engaged at home in a common object, and were acting together with great efficiency and harmony to contribute towards the comfort and security of the troops, by methodizing the spontaneous benevolence of the city and state of New York; obtaining information of the public authorities of the best method of aiding the War Department with such supplies as the regulations of the army do not provide, or the sudden and pressing necessities of the time do not permit the department to furnish; and striving to play into the hands of the regular authorities in ways as efficient and as little embarrassing as extra-official co-operation can be.

The delegation deprecated any notion of being troublesome to the War Department, but considered that some positive recognition of the existence and efforts of the associations they represented was essential to the peace and comfort of the several bureaux of the War Department. "The present," they wrote, "is especially a people's war. The hearts and minds, the bodies and souls of the whole people, of both sexes, throughout the loyal states, are in it. The rush of volunteers to arms is equalled by the enthusiasm and zeal of the women of the nation, and the clerical and medical professions vie with each other in their endeavours to contribute in some manner to the success of our noble and sacred cause. The War Department will, hereafter, therefore inevitably experience, in all its bureaux, the incessant and irresistible labours of this zeal, and the offer of medical aid, the applications of nurses, and the contribution of supplies. Ought not this noble and generous enthusiasm to be encouraged and utilized? Would not the Department win a still higher place in the confidence and affections of the good people of the loyal states, and find itself generally strengthened in its efforts, by accepting in some practical manner the services of the associations we represent, which are labouring to bring into system and practical shape the general zeal and benevolent activity of the women of the land in behalf of the army? And would not a greater economy of time, money, and effort be secured by fixing and regulating the relations of the Volunteer Associations to the War Department, and especially to the Medical Bureau?"

"Convinced by inquiries made here of the practical difficulty of reconciling the armies of their own and numerous associations in other cities with the regular workings of the Commissariat and the Medical Bureau, and yet fully persuaded of the importance to the country and the success of the war, of bringing such an arrangement about, the undersigned respectfully ask that a mixed Commission of civilians distinguished for their philanthropic experience and acquaintance with sanitary matters, of medical men, and of military officers, be appointed by the government, who shall be charged with the duty of investigating the best means of methodizing and reducing

to practical service the already active but undirected benevolence of the people towards the Army; who shall consider the general subject of the prevention of sickness and suffering among the troops, and suggest the easiest methods which the people at large can use to manifest their good-will toward the comfort, security, and health of the Army."

Further, the delegation pointed out, that "several such Commissions followed the Crimean and Indian wars;" but, they added, "the civilization and humanity of the age and of the American people demand that such commission should *precede* our second war of independence—more sacred than the first. We wish to prevent the evils that England and France could only investigate and deplore. This war ought to be waged in a spirit of the highest intelligence, humanity, and tenderness for the health, comfort, and safety of our brave troops. And every measure of the Government that shows its sense of this will be eminently popular, strengthen its hands, and redound to its glory at home and abroad."

In addition, the delegation urged several specific petitions.

1. They urged a greater rigour in the inspection of volunteer troops, fully assured that under the existing regulations "a great number of under-aged and unsuitable persons were mustered, who were likely to swell the bills of mortality in the army to a fearful percentage, and to encumber the hospitals and embarrass the columns."

2. They directed attention to the cooking of the volunteer and new regiments, and urged the importance of steps being taken to place it on a proper footing. The cooking of these forces, they said, was "destined to be of the most crude and perilous description, and no preventive measure could be so effectual in preserving health and keeping off disease, as an order of the Department regarding a skilled cook to be enlisted in each company of the regiments. The Women's Central Association in connexion with the Medical Boards, were prepared to assume the duty of collecting, registering, and instructing a body of cooks, if the Department will pass such an order, accompanying it with the allotment of such wages as are equitable."

3. The delegates next advert to the provision of nurses for the General Hospitals. "The committee," they say, "represent that the Women's Central Association of Relief have selected, and are selecting, out of several hundred candidates, one hundred women, suited in all respects to become nurses in the General Hospitals of the Army. These women the distinguished physicians and surgeons of the various hospitals in New York have undertaken to *educate and drill in a most thorough and laborious manner*; and the committee ask that the War Department consent to receive on wages these nurses, in such numbers as the exigencies of the campaign may require. It is not proposed that the nurses should advance to the seat of war, *until directly called for by the Medical Bureau here*, or that the Government should be at any expense until they are in actual service."

4. Finally, the delegates ask the Secretary of War to issue an order that "in case of need the Medical Bureau may call to the

aid of the regular medical force a set of volunteer dressers, composed of young medical men, drilled for this purpose by the hospital physicians and surgeons of New York, giving them such subsistence and such recognition as the rules of the service may allow under a generous construction."

It is only by an extended acquaintance with the condition of the volunteer forces throughout the war that the prescience of this remarkable letter of the delegates can be justly appreciated. The lesson taught to all powers engaged in war by the sanitary history of the Crimean War had been learned with unexampled aptitude.

The proposition of the delegates for the appointment of a Commission met with the warm approval of the Surgeon-General of the Federal forces, who, as the head of the Medical Bureau, supported the proposition. On the 23rd May the Delegation submitted to the Secretary of War a draft of the powers asked from the Government and of the specific objects to be sought by the proposed Commission. In this paper it is stated that the Commission "being organized for the purposes only of inquiry and advice, asks for no legal powers, but only the official recognition and moral countenance of the Government, which will be secured by its public appointment." It is also stated that "the Commission seeks no pecuniary remuneration from the Government. Its motives being humane and patriotic, its labours will be its own reward. The assignment to them of a room in one of the public buildings, with stationary and other necessary conveniences, would meet their expectations in this direction."

The *objects* of the Commission are said to be, generally, through suggestions reported from time to time to the Medical Bureau and the War Department, to bring to bear upon the health, comfort, and *morale* of the troops the fullest and safest teachings of Sanitary Science in its application to military life, whether deduced from theory or practical observation, from general hygienic principles, or from the experience of the Crimea, the East India, and the Italian wars. Its objects are purely advisory.

On the 9th June, 1861, the suggestions of the Delegation and of the Medical Bureau for the formation of a Sanitary Commission for the purposes above indicated were ordered to be carried out by the Secretary of War, Mr. Cameron; and on the 13th of June, were approved by the President, Mr. Lincoln. On the latter date, also, Mr. Cameron approved of a plan of organization laid before him; and he enjoined all persons in the service of the United States Government to respect and further the inquiries and objects of the Commission to the utmost of their ability.

The Commission was styled—"The Commission of Inquiry and Advice in respect of the Sanitary Interests of the United States Forces." The organization carried into effect was as follows:—

The commission was divided into two branches—one of *Inquiry*, the other of *Advice*, represented by two principal committees.

I. *Inquiry*.—This committee was subdivided into three sub-committees, with the following duties:—

1. Under the first committee's care came the suggestion of such

immediate aid, and such obvious recommendations as an intelligent foresight and an ordinary acquaintance with received principles of sanitary science would enable the Board at once to urge upon the public authorities. This committee had to occupy themselves with the question—"What *must be* the condition and want of troops gathered together in such masses, so suddenly, and with such inexperience?"

2. The second sub-committee had in charge directly or through agents, the actual exploration of recruiting posts, transports, camps, quarters, tents, forts, hospitals, and consultation with officers of every grade at their posts, to collect from them needful testimony as to the condition and wants of the troops. This committee had to deal with the question, "What *is* the condition of the Volunteer forces?" a question to be settled only by direct and positive observation and testimony.

3. The duty of the third sub-committee was to investigate, theoretically and practically, all questions of diet, cooking, and cooks; of clothing, foot, head, and body gear; of quarters, tents, booths, huts; of hospitals, field service, nurses, and surgical dressers; of climate and its effects, malaria and camp and hospital diseases and contagions; of ventilators, natural and artificial; of vaccination; of anti-scorbutics; disinfectants; of sinks, drains, camp sites, and cleanliness in general; of best method of economising and preparing rations, or changing and exchanging them. The question for this sub-committee to solve was, "What *ought to be* the condition of the new levies, and how would sanitary science bring them up to the standard of the highest attainable security and efficiency?"

II. *Advice*.—This committee was also divided into three sub-committees:—

1. A sub-committee in direct relation with the Government, the Medical Bureau, and the War department, and having for its object the communication of the counsels of the Commission and the procuring of their approval and ordering by the United States Government.

2. A sub-committee in direct relation with the army officers, medical men, the camps and hospitals, whose duty it should be to look after the actual carrying out of the orders of the War department and the Medical Bureau, and make sure by inspection, urgency and explanation, by influence, and all proper methods, of their actual accomplishment.

3. A sub-committee in direct relation with State Governments and with the public associations of benevolence; first, to secure uniformity of plans, and the proportion and harmony of action; and, finally, abundance of supplies in moneys and goods, for such extra purposes as the laws do not and cannot provide for. This sub-committee had indeed the task (a) of assimilating as far as practicable, conflicting State laws so far as they affected volunteers, and affected their efficiency with the general regulations of the United States; (b) of organizing between the benevolent associations throughout the country a common plan of action in respect to supplies, depots, and methods of feeding the extra demands of the

Medical Bureau or Commissariat, without embarrassment to the usual machinery; of inducing, in fact, serviceableness to the vague, disproportioned, and hap-hazard benevolence of the public, and (c) of looking after the pecuniary ways and means necessary for accomplishing the various objects of the Commission through solicitation of donations, either from State treasuries or private beneficence.

This gigantic scheme, under the presidency of the Rev. Dr. Bellows, was carried fully into action and worked effectually throughout the war.

The necessity of the Commission was abundantly shown by the state of things brought to light in the different volunteer camps, on the first inspections instituted by the Commission, and before the commencement of the first campaign. In concluding a report of a preliminary survey of the camps near Darlington, dated 9th July, 1861, the resident Secretary of the Commission wrote:—"He is compelled to believe that it is now hardly possible to place the volunteer army in a good defensive condition against the pestilential influences by which it must soon be surrounded. No general order calculated to strengthen and guard against their approach can be enforced with the necessary rigour. The captains, especially, have in general but the faintest comprehension of their proper responsibility; and, if they could be made to understand, they could not be made to perform the part which properly belongs to them in any purely military effort to this end. To somewhat mitigate the result is all that the Commission can hope to do." The Commission in the end did much more. The volunteer forces were in danger of falling to pieces from their own unwieldiness and unmanageable character. They ran the risk of disruption from the pestilential influences which were engendered by their ignorance, carelessness, and utter neglect of cleanliness. In the first report of the Commission to the Secretary of War, it is said "slovenliness is our most characteristic vice;" and the Commission look to at least one benefit likely to arise from the war:—"If," they say, "500,000 of our young men could be made to acquire something of the characteristic habits of soldiers, in respect to the care of their habitations, their persons, and their clothing, by the training of this war, the good which they would afterwards do as unconscious missionaries of a healthful reform throughout the country, would be by no means valueless to the nation."

That greater mischief did not befall the several forces than those which did happen was chiefly due to the efforts of the Sanitary Commission.

It was clear that the first thing to be done in establishing a better sanitary condition among the forces was to disperse the ignorance which prevailed respecting the health-requirements of bodies of men collected together under exceptional circumstances. This applied almost equally to the volunteer medical officers as well as combatants. The former called to novel duties, and without any works of reference in the English language which could aid him under the circumstances, could only look to experience for know-

ing his duties, and that experience could only be obtained by a sacrifice of the public good. One of the earliest tasks of the Commission was the preparation of a series of medical, surgical, sanitary monographs. The following is a list of these essays:—

- A.* Report on Military Regime and Therapeutics.
- B.* Directions to Army Surgeons on the Field of Battle.
- C.* Rules for Preserving the Health of the Soldier.
- D.* Report on the Use of Quinine as a prophylactic.
- E.* Report of Vaccination in Armies.
- F.* Report on Amputation.
- G.* Report on Amputation through the Foot and Ankle-joint.
- H.* Report on Venereal Diseases, with special reference to practice in the Army and Navy.
- I.* Report on Pneumonia.
- K.* Report on Continued Fevers.
- L.* Excision of Joints for Traumatic Cases.
- M.* Report on Dysentery.
- N.* Report on Scurvy.
- O.* Report on the Nature and Treatment of Fractures in Military Surgery.
- P.* Report on Nature and Treatment of Miasmatic Fever.
- Q.* Report on Nature and Treatment of Yellow Fever.
- R.* Hæmorrhage from Wounds.
- S.* Control and Prevention of Infectious Diseases in Camps, Transports, and Hospitals.
- T.* An introductory paper on the Employment of Anæsthetics in Military Surgery.

The directions to army surgeons on the field of battle are adopted from Mr. Guthrie's writings. Those for preserving the health of the soldier were written for general distribution in the Army, and were spread abroad broad-cast. The advice of the Crimean Sanitary Commission on Camping was also reprinted and distributed widely.

The medical paragraphs, with the exception of those marked B. and C., have been collected together in the handsome volume of essays noted at the head of this article. Without exception, all are admirable in conception and execution, and possess a permanent interest.

The history of the operations of the Sanitary Commission has still to be written. Much of what it has done may be gathered from its numerous reports, and the reports of its inspectors. But even these present but a disconnected and imperfect notion of the operations of this gigantic example of volunteer military hygienics. The narrative of the "works and purposes" of the Commission lying before us was written for the general public and for the purpose of fostering that vast sympathy which was the mainstay of its existence, by showing how well its work had been done. Happily the work of the Sanitary Commission is now virtually at an end, and we may hope that it will not long want a historian who can do justice to its labours. The vast scale of these may be judged by the following illustrations.

After the battle of Gettysburgh, upwards of 20,000 wounded men remained upon the field. The following is an account of the principal *supplementary* aid in clothing and sustenance furnished by the Commission.

Of Articles of Clothing, &c., viz.

	Estimated Value.
Of Drawers (woollen), 5310 pairs.....	\$9,292 50
" " (cotton) 1833 pairs.....	1,833 09
" Shirts (woollen), 7158.....	14,316 00
" " (cotton), 3266.....	3,266 00
" Pillows, 2114.....	1,268 40
" Pillow cases, 264.....	105 60
" Bed sacks, 1630.....	3,468 75
" Blankets, 1007.....	3,021 00
" Sheets, 274.....	274 00
" Wrappers, 508.....	1,498 00
" Handkerchiefs, 2659.....	319 08
" Stockings (woollen), 3560 pairs.....	1,780 00
" " (cotton), 2258 pairs.....	451 00
" Bed utensils, 728.....	182 00
" Towels and napkins, 10,000.....	1,500 00
" Sponges, 2300.....	230 00
" Combs, 1500.....	60 00
" Buckets, 200.....	75 00
" Soap (Castile), 250 pounds.....	50 00
" Oil silk, 300 yards.....	225 00
" Tin basins, cups, etc., 7000.....	700 00
" Old linen, bandages, etc., 110 barrels.....	1,100 00
" Water tanks, 7.....	70 00
" Water coolers, 46.....	280 00
" Bay, Rum and Cologne water, 225 bottles.....	112 50
" Fans, 3500.....	145 00
" Chloride of lime, 11 barrels.....	99 00
" Shoes and slippers, 4000 pairs.....	2,400 00
" Crutches, 1200.....	480 00
" Lanterns 180.....	90 00
" Candles, 350 pounds.....	70 00
" Canvas, 300 square yards.....	360 00
" Mosquito netting, 648 pieces.....	810 00
" Paper, 237 quires.....	23 70
" Pants, coats, hats, 189 pieces.....	96 75
" Plaster, 16 rolls.....	4 00

Of Articles of Sustenance, viz. :

Of fresh poultry and mutton, 11,000 pounds.....	\$1,540 00
" " butter, 6430 pounds.....	1,286 00
" " eggs (chiefly collected for the occasion at farm-houses in Pennsylvania and New Jersey), 8500 dozens.....	1,700 00
" " garden vegetables, 675 bushels.....	337 50
" " berries, 48 bushels.....	72 00
" " bread, 12,000 loaves.....	645 00
" Ice, 20,000 pounds.....	100 00
" Concentrated beef soup, 3800 pounds.....	3,800 00
" " milk, 12,000 pounds.....	3,125 00

	Estimated value.
Of Prepared farinaceous food, 7000 pounds.....	\$700 00
„ Dried fruit, 3500 pounds.....	350 00
„ Jellies and conserves, 2000 jars.....	1,000 00
„ Tamarinds, 750 gallons.....	600 00
„ Lemons, 116 boxes.....	580 00
„ Oranges, 6 boxes.....	230 00
„ Coffee, 850 pounds.....	272 00
„ Chocolate, 831 pounds.....	249 30
„ Tea, 426 pounds.....	383 40
„ White sugar, 6800 pounds.....	1,156 00
„ Syrups (lemon, &c.), 785 bottles.....	596 25
„ Brandy, 1250 bottles.....	1,250 00
„ Whiskey, 1168 bottles.....	700 80
„ Wine, 1148 bottles.....	861 00
„ Ale, 600 gallons.....	180 00
„ Biscuits, crackers, and rusk, 134 barrels.....	670 00
„ Preserved meats, 500 pounds.....	125 00
„ Preserved fish, 3600 pounds.....	720 00
„ Pickles, 400 gallons.....	120 00
„ Tobacco, 100 pounds.....	70 00
„ Tobacco pipes, 1000.....	5 00
„ Indian meal, 1621 pounds.....	40 50
„ Starch, 1074 pounds.....	75 18
„ Codfish, 3848 pounds.....	269 36
„ Canned fruit, 582 cans.....	436 50
„ „ oysters, 72 cans.....	36 00
„ Brandy peaches, 303 jars.....	303 00
„ Catsup, 43 jars.....	11 00
„ Vinegar, 24 bottles.....	3 00
„ Jamaica ginger, 43 jars.....	37 25
Total.....	\$74,838 52

At Fredericksburgh, for the wounded of General Meade's army, between the 9th and 23rd of May, 1864, the Commission, on the requisition of Congress, issued about 200 tons of sanitary stores, assigned to different hospitals from 160 to 225 relief agents, to act as nurses or minister to the wounded in other ways indicated by the surgeons, and established special diet-kitchens under the charge of ladies, from which were issued soup, coffee, stimulants, and other food to thousands of passing wounded. The Commission employed forty-four horse waggons in carrying them from Belle-Plain to Fredericksburgh; and detached seven four-horse waggons carrying food, stimulants, under-clothing, and surgical dressings, under the charge of its inspectors, to move with the army.

II.—*Seventh Report of the Medical Officer of the Privy Council, with Appendix, 1864. (Blue Book, 1865.)*

Mr. Simon's report is devoted to—(1) the continued Inspection of Vaccination Districts; (2) an Inquiry into the House Accommodation of the Labouring Classes in Rural Districts, and the Working of the Nuisances Removal Acts in England, conducted by Dr. Henry Julian Hunter; a Report on the Principal Parasitic Diseases of the Quadrupeds which are used for food, by Dr. Thudichum; and (4) certain Miscellaneous Proceedings chiefly with reference to outbreaks of contagious diseases.

1. *Vaccination.*—In the course of 1864, six hundred and nine vaccination-districts were inspected. The reports of the inspectors, added to similar reports which have been published during the four preceding years, "complete," says Mr. Simon, "for all England and Wales, an account of the working of our present vaccination laws; an account which, as it is founded on the fullest local inquiry into all details of the subject, offer such a basis as there never yet has been for effective legislation against small-pox."

2. *Dr. Hunter's Investigation.*—The inquiry into the house-accommodation of the labouring classes in several districts is a fitting complement to the investigation into the food of the poorer labouring classes, conducted by Dr. Ed. Smith for the Medical Department of the Privy Council, and published in Mr. Simon's Sixth Report. These two investigations have given an insight into the physical conditions of life among the poor and rural population such as had never before been obtained. There was no lack of information concerning the habitations and sustenance of the poorer inhabitants of towns. These had so often formed the subject of inquiry, the theme of popular discussion, and even of the novelist, that they were, or might be, as familiarly known as if each person had possessed an Asmodeus to unravel the secrets of our crowded alleys and back-slums. But little was actually known of the food and housing of the rural labourer. At times, strange facts came to light, which showed all too clearly that, at least in some spots, the poorer agriculturist was in no better case than the poorer urban operative. *Punch* gravely these scattered facts in the mind by that poignant cartoon in which the care-worn, half-fed, wretchedly-housed labourer obviously contemplates his landlord's healthy, rotund, well-fed, well-housed pigs. But that careful, accurate, and wide-extended inquiry requisite to a satisfactory knowledge of the physical conditions of life of the rural population, and to a correct appreciation by the stray facts which from time to time painfully attracted public attention, was wanting. Mr. Simon instituted such an inquiry; Dr. Ed. Smith completed one part of it, Dr. Hunter the other.

Nothing could have been better timed than Dr. Hunter's investigation. His report of its results had scarcely left the press before their great value was felt. They came just at the fitting time to show conclusively the need, as a question of public health as well as social morality, of the "Union Chargeability Bill," which it is to be trusted will become law as these pages are passing through the press.

Dr. Hunter's inquiry extended to 5375 occupied houses scattered in every district of the kingdom. The houses were selected so as to form *samples* of the rest. Every care was taken to reach this end, and the result of the inquiry may be fairly taken as presenting a general picture of the house-accommodation of the poorer agriculturists of the whole kingdom. It would be impossible, in the space at our disposal, to give any satisfactory *résumé* of Dr. Hunter's report. We must content ourselves with briefly noting some of Mr. Simon's observations upon it:—

"To the insufficient quantity and miserable quality of the house-accommodation generally had by our agricultural labourers, almost every page of Dr. Hunter's report bears testimony. And gradually, for many years past, the state of the labourer in these respects has been deteriorating—house-room being now greatly more difficult for him to find, and, when found, greatly less suitable to his needs, than perhaps for centuries has been the case. Especially within the last twenty or thirty years the evil has been in very rapid increase, and the household circumstances of the labourer are now in the highest degree deplorable. Except in so far as they whom his labour enriches see fit to treat him with a kind of pitiful indulgence, he is quite peculiarly helpless in the matter. Whether he shall find house-room on the land which he contributes to till, whether the house-room which he gets shall be human or swinish, whether he shall have the little space of garden that so vastly lessens the pressure of his poverty—all this does not depend on his willingness and ability to pay reasonable rent for the decent accommodation he requires, but depends on the use which others may see fit to make of their 'right to do as they will with their own' "(p. 9.)

Among the elements contributing to bring about this wretched state of things, Mr. Simon especially notes the influence of the Poor Law in its provisions concerning settlement and chargeability:—

"Under this influence, each parish has a pecuniary interest in reducing to a minimum the number of its resident labourers; for, unhappily, agricultural labour, instead of implying a safe and permanent independence for the hard-working labourer and his family, implies, for the most part, only a longer or shorter circuit to eventual pauperism—a pauperism which during the whole circuit is so near, that any illness or temporary failure of occupation necessitates immediate recourse to parochial relief; and thus all residence of agricultural population in a parish is glaringly an addition to its poor-rates."

Now one of the consequences of the desire to reduce the number of agricultural labourers is the diminution of house-accommodation. Dr. Hunter shows that notwithstanding increased local demands for them, destruction of houses, in the last ten years, had been in progress in 821 separate parishes or townships of England. The labourer unable, too often, to find house-accommodation at the seat of his labour, has to obtain it in neighbouring towns, often several miles away. A weary, exhausting walk takes largely from what should be his hour of rest, and diminishes his power of physical exertion; and in illness and impoverishment he is foisted upon the rates and charity of places which have had no profit from his labour.

There are, it is true, many spots which bear out the almost pro-

verbal fame of the English cottage home, but, says Mr. Simon:—

“From these brighter but exceptional scenes, it is requisite in the interests of justice that attention should again be drawn to the overwhelming preponderance of facts which are a reproach to the civilization of England. Lamentable indeed must be the case, when, notwithstanding all that is evident with regard to the quality of the present accommodation, it is the common conclusion of competent observers, that even the general badness of dwellings is an evil infinitely less urgent than their mere numerical insufficiency. For years, the overcrowding of rural labourers’ dwellings has been a matter of deep concern, not only to persons who care for sanitary good, but to persons who care for decent and moral life. For, again and again, in phrases so uniform that they seem stereotyped, reporters on the spread of epidemic disease in rural districts have insisted on the extreme importance of that overcrowding, as an influence which renders it a quite hopeless task to attempt the limiting of any infection which is introduced. And again and again it has been pointed out, that, notwithstanding the many salubrious influences which there are in country life, the crowding which so favours the extension of contagious disease also favours the origination of disease which is not contagious. And those who have denounced the overcrowded state of our rural population have not been silent as to a further mischief. Even where their primary concern has been only with the injury to health, often almost perforce they have referred to other relations of the subject. In showing how frequently it happens that adult persons of both sexes, married and unmarried, are huddled together in single small sleeping-rooms, their reports have carried the conviction that, under the circumstances they describe, decency must always be outraged, and morality almost of necessity suffer. Thus, for instance, in the Appendix of my last annual Report (p. 779,) Dr. Ord, reporting on an outbreak of fever at Wing in Buckinghamshire, mentions how a young man who had come thither from Wingrave with fever, ‘in the first days of his illness slept in a room with nine other persons. Within a fortnight several of these persons were attacked, and in the course of a few weeks five out of the nine had fever, and one died.’ And to this passage I appended the following foot note:—‘From Dr. Harvey, of St. George’s Hospital, who on private professional business visited Wing during the time of the epidemic, I received information exactly in the sense of the above report. Two cases of overcrowding which Dr. Harvey observed were the following. A young woman of nineteen, having fever, lay in a room occupied at night by her father and mother, her bastard child, two young men (her brothers) and her two sisters, each with a bastard child, ten persons in all. A few weeks ago thirteen persons slept in it. In a second house, in the downstairs room (whither they had been moved at the doctor’s orders from the bed-room where seven persons ordinarily slept) lay in one bed two young men, and in the other bed the sister with her bastard baby. The lads and women all had fever.’ Dr. Hunter’s report gives innumerable cases of that sort of over-crowding as consequent on the paucity and smallness of dwellings. Doubtless, in some instances, the evil is aggravated by the influence of local industries—straw-plaiting, glove-making, braiding, shoe-making, &c., which are followed as household occupations by young women who otherwise might be in service or married away from home:—but, though this influence may occur in aggravation, the main cause of the overcrowding is that which has already been discussed. It is true that neither the provisions of the Common-Lodging-Houses Act, nor the provisions as to the over-crowding contained in the Nuisances Removal Act, are all which could be desired for efficiency in the matters to

which they respectively relate :—but, apart from their technical insufficiency, and apart from the evident unwillingness which there is to apply them in country districts, it is certain that, while rural house-accommodation is on its present scale, not any statute could easily prevent the evils against which those provisions are directed. Here, as with regard to other main facts of the case, I may cite almost any part of Dr. Hunter's report; but, for perhaps the most general prevalence of the evil, I may refer to his account of the counties of Bedford, Buckingham, Warwick, Oxford, Worcester, and Northampton.

“By what enactments or other influences it may be practicable to amend the state of things which I describe from Dr. Hunter's report of the facts, is perhaps one of the most difficult problems in our present social economy. And while I feel that it would be irrelevant to the main object of this Report to enter upon the many and various considerations which that question involves, I also very deeply feel that, in an extreme degree and peculiar manner, it requires the wisdom of Parliament for its solution. Only most briefly and tentatively, therefore, would I suggest the direction in which it seems to me that the remedy has to be sought. First (and here I speak more positively than on any part of the subject) it seems indispensable that the Poor Law should no longer offer its present strong inducements for the dislodgment of agricultural labourers from places where their industry is employed. If the Union Chargeability Bill now before Parliament becomes law, those inducements will be in great part removed; and it may fairly be expected that, in prevention of further mischief, the results of the amendment will be very considerable. But if not absolutely necessary in order to arrest the mischief which is now in progress, at least to undo that which has been done, I suspect that more than the one amendment will be wanted. And the ulterior question which will, I think, have to be considered is this—whether all land which requires labour ought not to be held liable to the obligation of containing a certain proportion of suitable labourers' dwellings. A literal enforcement of that obligation might not indeed always be practicable or expedient. But if the principle of action were understood to be that which I have expressed, means (which need not here be discussed) would soon be found for fairly approximating to it in practice.”—(p. 13—15.)

3. *Dr. Thudichum's Report*.—The elaborate inquiry conducted by this gentleman, concerning the parasitic diseases of quadrupeds used for food, we trust will be published in a separate and readily accessible form. The following observations of Mr. Simon will make known the circumstances which gave rise to the inquiry, and its bearing upon the public health of this kingdom :—

“During the two years which have elapsed since the presentation of my fifth report, progress has in many respects been made in knowledge of animal parasites, and especially a mass of most important experience has been collected with regard to the *trichina spiralis*. This microscopical thread-worm has suddenly been shown to be of unsuspected importance to man. Happily not yet in England, but in several instances in Germany, the consumption of trichinous pork has been found at the root of local epidemics which of old would doubtless have been confounded with fever. It was by the alarming significance of one particular occurrence of that kind in Germany in the autumn of 1863, that my Lords were induced to order the present special inquiry. In the little town of Hettstädt in Prussian Saxony, the utilization of one trichinous pig (chiefly in festive celebration of the battle of Leipzig) had led to an epidemic of human trichiniasis where there were at least 158 sufferers, and no fewer than 28 deaths; and

not even the Schleswig-Holstein excitement of last year prevented Germany, high and low, from recognising almost tumultuously that the physiology of the trichina spiralis was a question of great importance to the nation. How important, may be judged from those pages of Dr. Thudichum's report which tell of the Hettstädt epidemic, or by reflection on the fact that doubts were currently expressed whether the use of so staple an article of diet as pork ought not to be absolutely discontinued. The importance of the subject is likely to be less in England than in Germany, because in England that consumption of uncooked or imperfectly-cooked pork which favours the ingestion of live trichinæ is vastly less frequent than in Germany; and moreover, up to the present time, trichinous swine do not seem to be of frequent occurrence in England. Still, in face of the circumstances to which I have referred, no apology will be needed for the very considerable extension which it has been thought desirable Dr. Thudichum should give to those parts of his report which relate to a parasite so dangerous and so easily overlooked. Indeed, altogether it has seemed expedient so to treat the whole subject of the parasitic diseases of stock, that the report may be referred to—and especially for medical purposes—as a monograph on that subject, and of course predominantly on its less familiar parts. It is with this view that the physiology of each parasite mentioned in the report has been discussed there at some little length; and that, in regard of trichiniasis—a subject almost new to this country—masses of detail are given (both from German experience and from Dr. Thudichum's own observations) to illustrate the habits of the parasite, and especially the laws of its reproduction and migration, and the circumstances under which man is likely to be infected by it, and the symptoms which result from its presence in the human body, and the ways in which trichiniasis may be distinguished from the diseases which it most nearly resembles." (p. 25.)

4. Of the *Miscellaneous Proceedings*, Dr. George Buchanan's report on typhus in Liverpool deserves special praise.

III.—*Report of the Commissioners appointed to inquire into the Condition of all Mines in Great Britain to which the Provisions of the Act 23 and 24 Vic. cap. 151 do not apply, with reference to the health and safety of persons employed in such Mines, with Appendix.* (Blue Book, 1864.)

This report contains the result of an official investigation into the health and safety of persons employed in mines not subjected to Government inspection and regulations. The inquiry, conducted by a Royal Commission, which included Dr. E. Headlam Greenhow and Mr. P. H. Holland among its members, occupied three years, and extended to all the great mineral mines—principally those from which copper, tin, lead, and iron are obtained—of Cornwall, of the Midland and Northern counties. The evidence given before the Commission, and the personal researches of the commissioners, afford a complete picture of the conditions of the mines and miners subjected to examination. The general result is somewhat anomalous at the present day, as showing, on the whole, a high degree of intel-

ligence among the operatives, and an apt appreciation of their needs by their managers, together with a singularly rude method of pursuing mining operations, and a deterioration of the health of the employed consequent thereupon of the gravest description.

The health of the copper, tin, and lead miners of Cornwall is greatly inferior to that of labourers engaged in agricultural and other open-air employments. When comparatively young, they almost invariably exhibit in their features and persons the unmistakable signs of debilitated constitutions:—"Their faces are sallow, they have an anxious countenance, and their bodies are thin." About mid-life the health begins rapidly to fail, decrepitude quickly follows, and, according to the local saying, "a person of fifty is old for a miner." At a time when his experience and skill would otherwise have made him a valuable workman, the miner becomes unfit for laborious work.

"The first symptoms of failing health amongst the miners," says the Report, "are weakness in the limbs in climbing the ladders and beating the bores, shortness of breathing, giddiness and pains in the head; their appetite fails, they are unable to take or to digest an adequate amount of food, and often suffer from sickness and vomiting. These symptoms are followed by harassing cough; much expectoration, sometimes of mucus, occasionally of blood; tightness of the chest, and failure of general strength. These affections precede total disability to work, followed by premature death. The delicate condition of the miners is the more striking when contrasted with the appearance of vigorous health generally presented by the women and children in the same districts, thus showing that the causes which operate vigorously upon the health of the miners are such as do not affect their wives and families."

Local medical practitioners would appear to have given little attention to the nature of the "miner's disease." It is most commonly registered as consumption; but the medical evidence shows that the malady is not identical with phthisis or tubercular pulmonary consumption. Tubercular consumption is not unknown among miners, but "by far the largest amount of mortality is due to other forms of lung disease of a bronchitic or asthmatic character." Rheumatic and dyspeptic, as well as pulmonary disorders, are common among the miners.

The deteriorated physical condition and unhealthiness of this class of workers is attributed—(1), to the early age at which miners commence working underground; (2), to the severity of the labour undergone, especially in deep mines, where ladders are the only means of access; (3), to working in a heated atmosphere, or in places which are draughty or wet from defective drainage; but, more especially (4), to the inferiority of the air which the men have to breathe underground. Further must be taken into account (5), the cold and damp at the surface, or in the shafts of the other parts of the mines, which operates with peculiar severity upon men in a state of perspiration, or exhausted from having worked in close places, or ascended ladders from great depths, and particularly upon those enfeebled in health and strength.

Dr. Peacock states, in a report to the Commission, that of the

men that he examined, 39 per cent. went to work when from ten to thirteen years of age, and 48 per cent. before the age of fourteen. He observes that "experience shows that, when persons in early life are exposed to injurious influences, they suffer from them more severely, and earlier fall under their influence, than if they had attained a more advanced age and greater constitutional vigour."

The vital statistics furnished to the Commission show that there is a large and progressive excess of mortality among the mining section of the male population of Cornwall. Taking the average mortality of the five years, 1849—53 inclusive, if it be assumed that the rate of mortality among the non-mining males at each decennial period of life be represented by 100, then among the miners it would be represented by 125 between the ages of fifteen and twenty-five years; by 101 between twenty-five and thirty-five; by 143 between thirty-five and forty-five; by 227 between forty-five and fifty-five; by 263 between fifty-five and sixty-five; and by 189 between sixty-five and seventy-five. It is to be inferred that the large and progressive increase of mortality among the miners is due to unwholesome conditions incident to their occupation, from the fact that it does not commence until these have had full time to operate.

A series of statistics, showing the comparative mortality of Cornish metal-miners and Northern coal-miners, clearly shows that the excessive mortality among the former is not caused by mere working underground in dark galleries. The mortality of the Cornish metal-miner is at all ages in excess of that of the Northern coal-miner. Between the ages of thirty-five and seventy-five the excess is very considerable, being almost as much above the rates prevailing among the coal-miners of the districts of Durham and Northumberland as above the rates among the non-mining male population of Cornwall.

From the returns of causes of death, it appears that the excessive mortality among the Cornish miners is caused by pulmonary disorders. Assuming that the rate of mortality among the males, exclusive of miners, is represented at each period of life by 100 (the date being based on the returns for the three years 1860-62), then that among the miners would be represented by 114 between the ages of fifteen and twenty-five years, by 108 between twenty-five and thirty-five, by 186 between thirty-five and forty-five, by 455 between forty-five and fifty-five, by 834 between fifty-five and sixty-five, and by 430 between sixty-five and seventy-five.

In Devonshire, the miners were found subject to disorders in every respect similar to those observed among the Cornish miners, and traceable to the same causes.

The lead-miners of Yorkshire and the northern counties have a more robust appearance than the Cornish miners, and there is a larger proportion of middle-aged and old men among them. The malady to which they are most subject is the miner's asthma, which they assign, together with impaired health, to bad air, powder "reek" (smoke), and "stour," or the dust of the mine. The majority of the northern mines are entered by dry levels, and the climbing ladders are rarely of length to tax the powers of the men injuriously. To the readiness of access to the mines, and the little stress upon

the strength from climbing ladders, and shorter working hours in the mines, Dr. Peacock assigns the better health-condition of the northern, as compared with the Cornish miners. The diseases met with among the former correspond generally with those observed among the latter, but the miner's asthma was a simpler form of disease than in Cornwall, and less frequently complicated by disorganization of the lungs, or with diseases of the heart.

In the northern districts, as in Cornwall, the metal-miners die in much larger and progressively-increasing numbers as the age augments, than the male population, exclusive of miners. The former also suffer much more severely than the latter from pulmonary disorders.

The metal-miners of Wales and Shropshire were found by Dr. Peacock to be far from a robust race. They suffer in the same manner, and from the same causes, as the Cornish and the North of England miners.

The rock-salt miners of Cheshire are a strong and healthy race. The spaces underground are large and well-ventilated, and the men are taken up and down the shafts in the buckets used to draw up the salt. The atmosphere underground and temperature are favourable. The miners are chiefly liable to colds, rheumatism, and neuralgia, arising probably from imprudent exposure in a half-naked state to the draughts of the shaft, when leaving work.

The men engaged in the Yorkshire ironstone mines are also a robust and healthy race, free from the premature decay and diseases of the copper and lead miners. Their robustness is ascribed (1), to the excellent ventilation of the mines; (2), to the more advanced age at which the men are accustomed to commence work underground, when the physical powers have attained true vigour; and (3), to the larger amount of animal food, and altogether more nutritious diet which they take, as compared with the copper and lead-miners.

IV.—*Supplement to the Twenty-fifth Annual Report of the Registrar-General of Births, Deaths, and Marriages in England.* 1864. 8vo, pp. 567.

This singularly important volume, the first of the same kind which has been published by the Registrar-General, has been compiled to show in detail, "from the consecutive records of ten years, the causes of death, and the comparative salubrity of every part of England and Wales." It shows the annual rate of mortality per 1000 in all the districts, counties, and divisions of England, during each of the decennials, 1841—50, and 1851—60. It also exhibits, in an elaborate series of tables, the deaths and the rate of mortality among males and females of the ages under 5 years, 5 to 10, 10 to 15, 15 to 20, 20 to 25, 25 to 30, 35 to 40, 45 to 55, 55 to 65, 65 to 75, 75 to 85, 85 and upwards; as well as the causes of death at all ages. By means of these tables an accurate knowledge of the salubrity of

the different registrated districts throughout England may be obtained. A work of this kind has long been needed, and it is to be hoped that every census will be followed by a like summary of the mortuary returns of the decennial periods.

We cite the chief results which are derived from these Tables:—

Causes of Death in Infancy: Age 0—1.—The causes of death are the functions are often not easily explored. Of the subjective symptoms no information is supplied by the little patient.

When the zymotic diseases are fully developed they are distinguishable in infants. Whooping-cough was fatal in infancy; but the bowel complaints were nearly three times as fatal as whooping-cough. Thus in the ten years 5027 boys and 4114 girls died annually of diarrhoea, dysentery, or cholera. From all zymotic diseases, 11,442 boys and 9956 girls died annually. Of scrofula, phthisis, and hydrocephalus, 3547 boys and 2723 girls died annually.

The convulsive diseases and other affections of the brain and spinal cord were fatal to 12,448 boys and 9171 girls of the first year of age annually. The diseases of the lungs were less fatal. 607 of the boys and 532 of the girls died by accidental or other violence yearly.

Nothing is known in our statistics about the still-born, as they are not at present registered, on the ground that it is difficult to distinguish them from abortions and miscarriages. But the difficulties are not insuperable; and in many ways the facilities of burying still-born children unregistered throw open the gate of temptation to crime.

Children under Five Years of Age: 0—5.—By the English Life Table, the mortality, it will be seen, decreases rapidly after the first year of life. The annual mortality of males per cent. in each of the first five years of age is 18·326, 6·690, 3·624, 2·416, and 1·799; of females 14·749, 6·436, 3·603, 2·450, and 1·785.

The mortality of males by the Life Table under five years of age is 7·014, of females 6·125. During the ten years 1851—60, the mortality of males of ages (0—5) in somewhat different proportions was 7·243 per cent., of females 6·274 per cent. The proportions of the first and second year's children are higher in the increasing population than they are in the Life Table.

When the mortality by each cause is treated of, we may take, to avoid fractions, 1,000,000 males living a year for basis; then to that number the annual deaths of zymotic diseases were by small-pox 1047, typhus, typhoid, and typhoid inclusive, 1401, measles 2847, whooping-cough 3246, scarlatina 4311, diphtheria 431, cholera, diarrhoea, and dysentery 5625; making, with others, 22,420 deaths out of 1,000,000 boys by all zymotic diseases, against 21,772 out of the same number of girls. Whooping-cough was more fatal to girls than it was to boys in the proportion of 4003 girls to 3246 boys; so was typhus or typhoid; all the other zymotics were more fatal to boys than to girls.

Cancer was fatal to few children; and the deaths were probably from soft cancer (*fungus hæmatodes*). There were many deaths from scrofula and phthisis. Hydrocephalus killed boys in the proportion of 2915 to 2162 girls; the other diseases of the brain in the proportion of 12,169 boys to 9479 girls. These brain diseases constitute a sixth part of the mortality of the young boys, and between one sixth and one seventh of the mortality of girls.

Convulsions in infancy is the capital head under which these affections accumulate. Like diarrhœa, convulsion is the result of teething, local irritations, poisons, zymotic action in the early stage, before the development of its characteristic symptoms. The nerve-force is thrown into motion by a great variety of causes; and the convulsion of the muscles is so striking a symptom that it overwhelms all others, some even more dangerous in their essence.

The heart and lungs—the great seats of the circulatory and respiratory systems—are intimately connected. Death ensues when the heart does not beat. The stoppage of the breath, or anything to prevent the access of oxygen and the expulsion of carbonic acid, is fatal. Thus from the affections of the respiratory system we have a mortality of 11,296 boys and 9499 girls per million. The lung and the brain diseases of girls are equally fatal.

The diseases of the digestive organs produced a mortality of 1470 boys and 1121 girls.

The maladies of the urinary organs, of the organs of generation, of the joints (excluding scrofula), and the skin are rarely fatal in childhood.

The violent deaths are nearly as fatal as the idiopathic diseases of the digestive organs, for 1451 boys and 1138 girls per million living died annually by violence.

Under other causes are included premature births, atrophy, and debility, malformations, teething, as well as cases in which the causes are unspecified. There are 16,579 annual deaths to a million boys, and 13,885 deaths to a million girls, from a group of causes, including malformations, premature birth, atrophy, debility, and other ill-defined developmental diseases.

Mortality of Children (0—5) in different Districts.—Death in childhood is an unnatural event, inasmuch as the regular series of development of the human structure from the germ-cell to the perfect man in his prime, and in his last declining stage of existence, is interrupted. But life at all ages depends upon so many conditions, and is exposed to so many risks, that out of given numbers living some die at every age, and we can only take for a practical standard the lowest authenticated rates of mortality.

Thus in the 63 healthy districts of England the annual mortality of boys under five years of age was at the rate of 4·348, and of girls 3·720 per cent.; the mean being 4·034.

Twenty-eight districts have been selected, showing the low annual rate of mortality 3·348 for the mean of the rates of the two sexes: the boys dying at the rate of 3·576, the girls at the rate of 3·120.

The twenty-eight districts are found in all the regions of England

and Wales, from the northern limits of Northumberland to the New Forest on the Southampton Waters.

The mortality was at the annual rate of 2·317 in Bellingham; 2·593 for boys, 2·040 for girls. This rate is slightly exceeded in the adjoining district of Rothbury, also on the border of Northumberland; and in Bootle, north of the Duddon on the coast of Cumberland.

The mortality among the families of the British Peerage has been investigated with much care and ability by Mr. A. H. Bailey and Mr. A. Day. They confined their investigation to the peers, the children of peers, and the children of the eldest sons of peers living in the present century. The numbers existing on December 31, 1855, were 4282; 2283 males, and 1999 females.

The mortality of peers' children under five years of age was at the rate of 2·069 per cent.; among boys under five years of age 2·227, girls 1·882.

The number of facts for the peerage is small; as the deaths of boys were 274, of girls 196; or 470 in the aggregate. For Bellingham the deaths of boys in ten years were only 112, of girls 82; 194 in the aggregate. The districts of the lowest mortality are very thinly peopled, but there is no reason to suspect that any of the deaths are unregistered. And the mortality only proceeds gradually step by step up to 3·500 in many other districts.

Dr. Joseph John Fox, in a valuable paper on the vital statistics of the Society of Friends, found that by the returns in the "Annual Monitor" the mortality under five years of age in 1842—52 was at the rate for boys of 3·190, of girls 2·383. This mortality differs little from the mortality of the peerage. Another return makes the mortality of the boys of Friends 5·598, of the girls 4·733.

The mortality of 1087 children of the clergy has been investigated by the Rev. John Hodgson, M.A., who procured returns in 1829 and 1858 from the parents; the mortality of the boys was at the rate of 3·729, of the girls at the rate of 2·302 in the years under observation; making the mean mortality of the sexes 3·027.

Very different are the rates of mortality among children in one hundred and fifty-one districts; where the lowest mortality among the boys is at the rate of 7·084, and the highest at the rate of 13·741 per cent. annually. The mean mortality of the districts was for boys 8·593, for girls 7·432, for both sexes 8·013.

These mean rates are obtained by adding up the district rates, and dividing by the number of districts.

The population of children in the one hundred and fifty-one districts was 1,391,420 in 1861; and the annual deaths at the rate (3·348) of twenty-eight healthy districts would be 46,585; while at the mean rate (8·013) it would be 111,494. Thus there is an annual sacrifice of about 64,909 children's lives by various causes in one hundred and fifty-one districts of the kingdom.

The mean annual mortality of children under five years of age was 10·022 per cent. in Sheffield, 10·149 in East London (City), 10·203 in Coventry, 10·219 in Nottingham, 10·246 in Whitechapel, 10·277 in Leeds, 10·480 in Wolverhampton, 10·852 in St. Giles

(London), 11·725 in Manchester District, and 13·198 in Liverpool District.

There is no doubt great negligence on the part of the parents, great ignorance of the conditions on which health depends, and great privation among the masses of the poor, but there is no reason to suspect that any great number of the infants in these districts fall victims to deliberate crime; yet the children of the idolatrous tribe who passed them through the fire to Moloch scarcely incurred more danger than is incurred by the children born in several districts of our large cities.

A strict investigation of all the circumstances of these children's lives might lead to important discoveries, and may suggest remedies for evils of which it is difficult to exaggerate the magnitude.

The weaklier lives, it is said, are, under this state of things, cut off; but it must also be borne in mind that many of the strongest children are wounded and are left weakly for life.

Childhood: Age 5 and under 10 years: 5—10.—The child is at this age able to walk and to talk; his forces are greater, and his hold on life is firmer than it was. The rate of mortality declines with every year of age; and during the period of ten years 1851—60 it was at the rate per cent. of '851 for males, '842 for females. The mean rate of mortality for the 25 years 1838—1862 was '883 and '876; so that in the years 1851—60 there is a reduction in the rate of mortality per cent. of boys '032 and of girls '034.

Zymotic diseases cause more than half ('451 boys and '478 girls per cent.) of the mortality at this stage of childhood; and of the zymotic diseases, scarlatina and fever (typhus, typhoid, and typhina, including typhoid and infantile intermittent fever), were much the most fatal. The mortality from scarlatina is less by half than it was in the previous age, but it remains much more fatal than small-pox, measles, whooping-cough, diarrhoea, and other maladies of this class. Diphtheria, which has now taken its place among the fatal diseases of England, in this respect resembles scarlatina.

Scrofula, tabes, phthisis, hydrocephalus, and diseases of the brain contribute largely to the mortality of this age; so do diseases of the lungs.

In one hundred and sixty-three districts the rate of mortality in both males and females was less than '700; among boys it was '388 in the district of Shipston-on-Stour, and '398 in Bedale, '435 in Cranbrook, '492 in Sevenoaks, '414 in Petworth, '489 in South Stoneham, '441 in Andover, '411 in Ware, '498 in Royston, '470 in Henley, '474 in Brixworth, '499 in Shaftesbury, '458 in the Scilly Islands, '495 in Ludlow, '483 in Martley, '474 in Pershore, '414 in Billesdon, '476 in Leyburn, '453 in Askrigg, '446 in Reeth, '469 in Bellingham, '434 in Presteigne.

Among girls the lowest rates of mortality were recorded in the Scilly Islands '299, Billesdon '396, and Reeth '348.

Very different rates of mortality prevailed in thirty-one districts, where the mortality of males and females exceeded 1·000 per cent.: the mortality of boys was 1·256 in Bristol, 1·277 in Manchester,

1·311 in Merthyr Tydfil, 1·367 in St. James' Westminster, 1·391 in St. Giles (London), and 1·457 in Liverpool. The mortality of females in the same districts was 1·014, 1·220, 1·314, 1·065, 1·083, and 1·433. To boys and girls of this age the Liverpool district was the most fatal.

Boyhood: Age 10—15 years.—This is the age of puberty; and the mortality decreasing from birth is at its lowest rate about the middle of the period; among boys the mortality in the ten years was at the rate of ·488, among girls at the rate of ·506 per cent. Among 1000 boys less than 5 die every year. As boys actually ill are not sent to public schools, an average of 1 death in 200 boys there would imply an exceptionally high rate of mortality.

The deaths of boys arise chiefly from injuries under the class of violence ·077, from consumption ·076, from fever ·069, and from scarlatina ·046.

Girls die much less frequently of violence, including burns, than boys; their mortality from this class of causes is only ·016. Upon the other hand they suffer much more than boys from consumption; which at this early age shows its predilection for their sex. How much organization, in-door life, and compression of the chest, interfering with the free action of the breathing organs, have to do with the excess of consumption in girls, it is difficult to say.

Of the salutary effects of free breathing in the open air there can be no doubt, and if they are studied, it is probable that among them will be found the reduction of the mortality by consumption from ·129 to a figure nearer that of boys, ·076, which is still much higher than it ought to be.

Fever, scarlatina, and diphtheria are more fatal to girls than boys.

The mortality of boys of the age 10—15 in all England is ·488; but there are ninety-five districts in which the mortality is below ·350; in the Stockbridge district their mortality was at the rate of ·198; in Catherington ·144; in Penrith ·220; in Easingwold and in Bedale ·215.

In Merthyr Tydfil the mortality of the boys was 1·089: at this age it is the highest rate in England. Sedbergh ·936, Abergavenny ·877, and Leek ·861, stand next on the list. The mortality exceeds ·700 in Macclesfield, Congleton, Wigan, Auckland, Easington, Houghton-le-Spring, Chester-le-Street, Neath, Llanelly, Aberayron, and Festiniog.

Occupation at this age, plays a part in the mortality; and in the mining districts many of the boys are killed under ground.

Youth: Age 15—20 and 20—25.—Growth continues through the whole of these ages; and the body attains its full strength at 25, which is near the average age of first marriage in England.

Men are now exposed to the full influence of their occupations; but the effect is sometimes only developed later in life, as is the case in respect to Cornish miners.

The mortality after the age of 15 increases; thus proving that the vital tenacity of men is not proportional to their growth, size, vigour, and intelligence.

The mortality at the age 15—20 in all England is at the rate of '669 per cent. for men, and '738 for women; at the age of 20—25 the mortality is '883 for men, and '853 for women.

The zymotic diseases, which are exceedingly infectious, and occur only once in life, such as measles and whooping-cough, have generally been undergone before the age of 20. Scarletina and diphtheria also subside as the persons remaining unaffected diminish. Small-pox is, however, more fatal at the age 20—25 than it was at 10—15, implying, perhaps, that vaccination was not so common 20 years as it was 15 years ago, rather than any diminution in the efficacy of vaccination as age advances. Fever is the most fatal zymotic disease at these ages; that it is more fatal than it is at the age 25—35, is due probably to the exposure of immigrants in the towns to the various forms of typhine, and to their subsequent comparative immunity from its effects. The mortality from fever of men of the two ages was '086 and '085; of women '103 and '078. Young girls are more exposed than young men to the sources of typhoid fever, which Dr. Murchison has aptly designated pythogenic fever. The mortality by violent deaths was '086 and '095 among men of these two ages; and only '016 and '013 among women. But one cause of death at these ages is peculiar to women; the mortality from childbirth, including metria or the fever of that name, is '014 and '061. These deaths thrown in do not compensate for the excess of deaths by violence among men; but phthisis is much more prevalent among young women than it is among young men, that it more than compensates at 15—20 for the excessive deaths by violence among young men. The mortality by phthisis at the two ages was '240 and '406 for men, '352 and '429 for women. Half the deaths of young women at these ages are by consumption.

The mortality of males at the age 15—20 is lower than '400 per cent. from all causes in thirty districts of the country, and exceeds '800 in eighty-five districts. The mortality of districts is swollen at this age and the age following by deaths in hospitals, to which unmarried men often resort in great numbers. Several Welsh and other rural districts also figure here.

The mortality of males from all causes at the age 20—25 is below '500 per cent. in twelve districts; and above 1'200 in 42 districts.

Reproductive: Age 25—35.—At this age 67 in 100 of the men are husbands and 67 in 100 of the women of England are wives, or 2 in 3; and a considerable proportion of them are parents. By early death 2 of 100 men are left widowers and 3 of 100 women are widows.

The mortality of men at this age is '957 per cent., of women '992 per cent. Fever is the chief zymotic disease; '067 for men and '063 for women; but the mortality by diarrhoea, dysentery, and cholera is growing greater; it is '024 for men and '027 for women. Phthisis is the great preponderating malady; the mortality by it was '403 for men and '458 for women. By violence the rate was '100 for men, '013 for women; but at this age childbirth induces a mortality of '089; thus making the mortality under these two heads greater than the mortality of men by violence.

The diseases of the brain, of the heart, and of all the important organs begin to grow more fatal; but the organs give way much more frequently at later ages.

In thirty-three districts the mortality of men was at rates below .650 per cent.; in Kettering the mortality was .508, Thrapstone .597, Evesham .557, Pershore .597, Billesdon .573, Bourn .545, Gainsborough .579, Wetherby .588, Tadcaster .538, Pocklington .562, Reeth .569, Easington .581, Longtown .596.

The mortality exceeded the rate of 1.500 per cent. in eleven districts; it was 1.995 in Gravesend, 1.719 in the Scilly Islands, 1.547 in Aberayron, and 1.596 in Liverpool. The high mortality 2.010 in Barnet is due to a lunatic asylum; in Alverstokey, East Stonehouse, and the Medway, to hospitals.

Manhood: Age 35—45.—A large proportion of the men and women of this age are settled, are married, and have children. Their occupations for life are fixed, and the effects of workshops on health and disease are sometimes perceptible.

The mortality of men is at the rate of 1.248 per cent. The mortality by fever .065 is lower than it is at any other age, as the number of susceptible persons has diminished, and the diminution is not yet counteracted by the high rate of mortality from advancing age among those attacked. Cholera, diarrhoea, and dysentery .032, and other zymotic diseases (.052) are fatal to some extent; but by all zymotic diseases the mortality is .160. Consumption is still the great fatal disease .400; as to it a third of the deaths are referable. But the brain, heart, lungs, stomach, and kidneys, show signs of wear; and the mortality from all the classes of monorganic diseases is double that of the previous period. Violent deaths kill at the rate of .115; as like accidents grow more fatal.

The mortality of females 1.215 is not quite at so high a rate as the mortality of males; either in the class of zymotics, or in the brain, lung, and kidney diseases. The mortality by violence is only .018 among females against .115 among males; but this is nearly counterbalanced by the deaths of women from childbirth .090, and ovarian dropsy .021. Cancer, a terrible disease developing with years, now makes an impression in the catalogue of women's diseases; its mortality is for men .018, for women .059 per cent.

While the mortality of men in England was at the rate of 1.248 per cent., there were seventy-one districts in which the rate of mortality was below .800. The rate was .507 in Holsworthy, .516 in Market Bosworth, and .539 in Longtown.

The mortality exceeded 1.800 per cent. in twenty-four districts; nearly all of them in large towns, and nearly all of them containing large hospitals or large lunatic asylums, where many patients die at these ages, and at the three ages following.

The mortality of men in London, for example, at this age is 1.629 per cent.; and .303 of that mortality, little less than a fifth of the whole, takes place in the great hospitals. The mortality of London women at the same age is only 1.281; and only .127 per cent. of them die in the hospitals.

Manhood: Age 45—55.—At this age the intellect is developed and athletic power declines: the reproductive age in woman is nearly over.

The mortality of men was at the rate of 1·796 per cent.; to which zymotic diseases, chiefly fever and cholera, contributed ·207; phthisis ·383; the chief increase arising under the heads of monorganic diseases of the brain, heart, lungs, stomach, and kidneys, covering ·912 of the mortality. By violent deaths ·137 died.

Women, except a few, have past the child-bearing age; and the mortality from this cause is inconsiderable. From all causes it is 1·520 per cent.; comprising from consumption ·312, that of males being ·383; of brain and nerve diseases ·199 men and ·168 women die; of heart disease and dropsy ·190 men and ·206 women; of diseases of lungs ·309 men and ·206 women. It is probable that the excessive mortality of men at this age—such as miners—is from the bad air and dust which they breathe at work. The diseases of the urinary organs are more fatal to men; those of the generative organs, including ovarian dropsy, to women. Of cancer, the mortality is ·042 for men and ·128 for women.

A considerable portion of the higher mortality of men is due to the excess in their deaths by violence; by which the mortality is ·137 for men, and ·027 for women.

While the mortality of men in England is 1·796, there are twenty-three of its districts in which the rate of mortality is below 1·000; in Ringwood it is ·791, Holsworthy ·783, Caxton ·894, St. Faith (Norwich) ·821, Malmesbury ·889, Winchcomb ·890, Billesdon ·828, Leyburn ·886, and Rhayader ·890.

Upon the other hand, in twenty-six districts the annual mortality of men exceeded 2·600 per cent. Fifteen of the districts are in London; and the following five contain neither hospital nor lunatic asylum: St. James Westminster 2·631, St. Giles 3·031, London City 2·908, Whitechapel 3·082, and St. George-in-the-East 2·829.

The lunatic asylums at this age disturb the rate of mortality; thus the Colney Hatch County Asylum raises the mortality of men in Barnet, one of the healthiest districts of Middlesex, to 3·846 per cent.

The mortality of men in all London is at the rate of 2·468 per cent.; of which about ·365 is in the hospitals.

Maturity: Age 55—65.—The mortality of men of this age was at the rate of 3·086 per cent.; to which zymotic diseases, including fever and cholera, contributed ·313; consumption ·333; diseases of the brain ·410, heart ·413, lungs ·662, stomach ·303, kidneys ·094.

The mortality of women was at the rate of 2·701 per cent. The zymotic diseases, consumption, brain affections, lung affections, stomach and kidney affections, were less fatal than in males. The rate of deaths by violence was ·038 in women, ·161 in men. Upon the other hand the rate by cancer was ·093 for men, ·185 for women; by diseases of the generative organs ·001 for men, and ·031 for women, including ovarian diseases.

In forty-nine districts the mortality of men was below 2·000, or less by 1·086 than the average; in Westhampnett 1·702, Kingsclere

1·757, Tingoe 1·460, Loddon 1·752, Depwade 1·653, Scilly Islands 1·505, Wheatenhurst 1·778, Longtown 1·741, Bootle 1·606.

To men of this age, Alston was the most fatal district in England; their mortality there was at the rate of 6·800 per cent.; then followed Reeth 5·060, the City of London 5·014, St. Giles, London, 5·243, Whitechapel, containing a hospital, 5·483, Manchester 5·266, and Liverpool, 5·350.

Maturity : Age 65—75.—The mortality of men at this period of life is more than double their mortality in the previous decenniad. In the ten years it was 6·533 per cent.; of which ·579 was by zymotic diseases; ·150 by cancer; ·239 by phthisis; ·983 by diseases of the brain; ·871 by diseases of the heart; 1·342 by lung diseases; ·484 by diseases of the digestive organs; ·245 by diseases of the urinary organs; making the aggregate mortality by local or monorganic diseases 3·964. The mortality by violence of various kinds was at the rate of ·181 per cent.

The mortality of women at this age was 5·866, or less by ·667 than that of men. Fever was somewhat less fatal to them than to men; so also were phthisis and all the pulmonary diseases. The mortality of women by kidney, &c. diseases was ·049, of men ·245; by violence, of women ·072, of men ·181. Upon the other hand, cancer killed women at the rate of ·235. Uterine and ovarian diseases at the rate of ·032.

In thirteen districts of England the mortality of men of the age 65—75 was below 4·000 per cent.; it was 3·548 in the Scilly Islands, Flegg 3·667, Rothbury 3·831, Easthampstead 4·427, Henstead 4·258, Thetford 4·444, Cricklade 4·467, Tetbury 4·369, Sedbergh 4·479, Easingwold 4·253, Brampton 4·142, Bridgend 4·487, Knighton 4·455.

In Alston the mortality at this age was 11·731 per cent.; in Reeth 9·524; and it exceeded 9·000 in twenty-one districts, including Alston and Reeth; namely, in thirteen London districts, in Birmingham, Liverpool, Manchester, Leeds, Sheffield, and Newcastle-upon-Tyne.

Ripeness : Age 75—85.—The mortality of men at this advanced age is at the rate of 14·667 per cent., of which nearly the half is by causes undistinguished: for the functional symptoms become obscure as age advances. Only 1·004 of the deaths were by recognised zymotic diseases, ·173 by cancer, ·098 by consumption. The chief mortality was by diseases of the brain 1·709, heart 1·241, lungs 2·109, stomach ·525, kidneys ·427. By violence ·225 died.

The mortality of women was at rate of 13·434 per cent.; cancer being more, phthisis less, fatal to women than it was in men; of the total mortality 5·031 was by monorganic diseases; while in men the mortality by these diseases was 6·067 per cent.

In descending to other districts the mortality was below 12·000 per cent. from all causes in fifty districts, and above 17·000 per cent. in seventy districts, including twenty-four London districts, and the districts of the chief large towns.

Old Age : Age 85 and upwards.—This may be called the monumental age; the cup of life is full of years; and the mortality of

men is at the rate of 31·008, of women at the rate of 28·956 per cent. The forms of disease are imperfectly developed; the symptoms are obscure; and in three-fourths of the cases the deaths are simply referred to age, and natural decay, or some of the maladies which have not been inserted in the synoptic tables. Cholera did not disdain to destroy a certain number of these lingering lives; some of the men were killed by violence; some of the women were burnt by their clothes taking fire; and many died of recognised monorganic diseases of the brain and chest.

Annual Rate of Mortality at all Ages without Distinction.—The mortality of the various populations of the world is generally stated as one in so many, or as so many per cent. per annum. The latter result is the ratio of the deaths at all ages to the living at all ages.

Now it is evident from the preceding tables that the proportion of deaths to a given number living varies to a great extent with the ages of the living; in the first five years of age the mortality is at the rate of 7·243 per cent. for boys, at the age 10—15 it is 4·88, at 55—65 it is 3·085, at 75—85 it is 14·667 per cent. The mortality of the two sexes also varies, so that, independently of other causes of variation, the mortality of different populations will differ according as they consist of numbers in various proportions at the ages at which the mortality is high or low.

When the population is sustained by an uniform number of births, the number living at each age is regulated solely by the law of mortality, reducing the numbers year by year, until each annual generation is extinguished.

The laws of mortality may vary infinitely, it is conceivable, so as to yield the same mean lifetime, and the same rate of mortality. Thus by the English Life Table 1,000,000 children born alive die off so as to leave survivors in every year of age up to the 109th, when the last of the generation dies off; the mean lifetime is 40·858, and one in 40·858, or 2·447 per cent. of the population so constituted die annually. If every one of the 1,000,000 children lived 40·858 years, and died at the end of the term, the mean lifetime would be 40·858 years, and it will be evident that the mortality would be at the rate of 1 in 40·858, or 2·447 per cent. per annum. Yet how different are all the circumstances! How different are the conditions of existence! How different is the law of mortality!

The rate of mortality in England was not 2·447, but 2·245 per cent. per annum, during the period when the facts were collected upon which the Table is based. Thus 1 in 44·54 died; while the mean lifetime was 40·858.

Assuming the prevalence of the same law of mortality, the rate calculated on the mean population, and the deaths at all ages, is lower in a rapidly increasing population than it is in a stationary population, because the mortality at all ages from about 4 to 54, is lower than the mean mortality of the whole normal population; and while a regular increase of population has the effect of increasing the proportion of children under 4 years of age, who die off quickly, it has also the effect of still further increasing the pro-

portion of the living at the ages 4 to 54 and of diminishing the proportions of the old people, whose rate of mortality is high.

In the healthy districts of England the normal mortality is 2·059 for males, and 2·022 for females by the Life Table; while it is 1·772 for males and 1·733 for females, as deduced from the ratio of deaths at all ages to the living at all ages. This is the rate of 17 deaths per 1000 of the population, which is so often and so fairly quoted, as a standard of comparison applicable to increasing populations.

There is another disturbance of the proportions living at ages more or less mortal, by immigration and by emigration. Thus the general effect of immigration into towns is to reduce their rates of mortality, by increasing the proportion of the living at ages of less than the mean mortality of the people of the place. The bulk of the emigrants to towns from the country are probably in good health, but a certain number of sick resort to the town hospitals; upon the other hand, of the emigrants, some are consumptive, seeking health in the country and abroad, or returning home to die; but the emigrants are less numerous in the aggregate than immigrants, and so far have less effect on the mortality.

V.—*Transactions of the Epidemiological Society of London.*
Vol. II., Part I. 8vo, pp. 246. With Appendices.
Hardwicke. 1865.

This work is rich in the special subjects which occupy the attention of the Epidemiological Society. Two papers on an anomalous form of eruptive disorder, one by Dr. B. W. Richardson, the other by Dr. Babington, are of peculiar interest. It often happens that practitioners are confronted with a form of exanthem, which, while having several bonds of relationship with scarlet fever and measles, is neither the one disease nor the other. Dr. Richardson describes several cases of this character. Now, this disease, he says, must hold one of four positions :—(1) It is a disease holding place between measles and scarlet fever, imitating both, but distinct of itself; (2) or, it is a disease compounded of the two diseases, measles and scarlet fever; (3) or it is an imperfectly recognised form of scarlet fever; (4) or it is another disease emulating scarlet fever, but distinct from it. Dr. Richardson holds that the last proposition best suits the facts of the case; he suggests that the malady should be termed *Rosalia idiopathica*. The disease possesses the following characteristics, which serve, he thinks, to distinguish it from allied maladies. It is not contagious (the poison being fixed in character) except by direct inoculation. It shows a tendency to produce disease of the kidney, or uræmia. This was the fact in all his cases, 25 in number. Unlike scarlatina and measles, it is variable in its course; it may terminate at once by active vomiting or purging, by which offending matter is removed from the alimentary canal; it may continue until it terminates in death. It is probably excited, in the stomach, by the irregular digestion of some particular forms of food, which yield in

the digestive processes products of the fixed organic acid types, which products are absorbed from the canal into the blood.

In the course of 1864 an anomalous exanthem, clearly allied to the class of cases described by Dr. Richardson, was epidemic in the metropolis. Dr. Babington directed the attention of the Society to this outbreak, and thus describes the disease :—

“It is a papular eruption, in many respects resembling rubeola ; but in all the cases seen by me, there has been satisfactory evidence that the individuals affected have previously had that disease. It is ushered in during several days by constitutional disturbance, headache, loss of appetite, febricula, coryza, and sneezing. When the eruption appears, the papulæ are less distinct than those of rubeola, are not arranged in crescentic clusters, and appear on the face and trunk, but not upon the upper or lower extremities, or at most very slightly. The general febrile symptoms are somewhat relieved by the external eruption, which is most vivid on the second day of its appearance, but does not entirely disappear until the third.

“There is more constitutional disturbance in proportion to the extent of the eruption than in rubeola, but the convalescence is more speedy and complete. From roseola it differs in not being symptomatic of any other disorder, and in being papular in character, the papulæ appearing in circumscribed patches, which are more dusky in hue than the diffused efflorescence of roseola. There is also more marked congestion of the head, and less irritation of the stomach ; moreover, the complaint runs a more definite course. From scarlatina the disease differs, inasmuch as the eruption is essentially papular—not a general efflorescence—and that there is little or no soreness and no ulceration of the throat.

“The prognosis of the affection is favourable ; and, so far as has been hitherto noticed, it has not been followed by any morbid condition of the kidneys.

“With respect to treatment, the feverish symptoms by which it is preceded and accompanied are best met by cooling salines, diluent beverages, and a light diet. Gentle aperients are occasionally required, but, during the eruption, spontaneous diarrhoea sometimes occurs. The debility during convalescence, which is considerable, indicates the employment of tonics and stimulants.

“From the general resemblance of this eruptive epidemic to rubeola, I would propose to designate it ‘Rubeola notha,’ or bastard measles.”

Dr. Babington’s term has passed into general use as best meeting the needs of the case ; while probably those anomalous cases which most nearly approach scarlet fever in character, would be best designated “*scarlatina notha*,” or bastard scarlet fever. Drs. Mackay, R.N., and Pickthorn, R.N., describe an outbreak of an anomalous eruptive disorder, similar to that observed in London, which prevailed in Malta in 1861.

Mr. Hunt discusses in a very practical spirit certain endemic affections observed in schools, factories, and workhouses ; and Dr. George Buchanan gives a careful history of the outbreak of typhus in Lancashire during the cotton famine. A report of a local eruption of typhoid fever enlists a melancholy interest, as the last work of the late Dr. Edward R. Harvey. Mr. Marson, in a brief report of a special trial of *Sarracenia purpurea* in smallpox, shows its worthlessness. Dr. Ord describes a very unusual epi-

demic of jaundice at Rotherham, in Yorkshire, in 1863. Few such outbreaks are on record, and their etiology is very obscure. The honorary secretary of the society, Mr. Radcliffe, gives a detailed report of the epidemics prevailing in England in 1862. The important questions of syphilis in the army, and of revaccination among the forces, are discussed by Dr. Thomas Bowen, while Dr. Dickson, R.N., the Medical Inspector of the Customs, deals with the question of syphilis in the navy.

The papers relating to the epidemiology of foreign countries are of great value. Dr. Swarbeck Hall, of Hobart Town, furnishes a systematic account of the epidemic diseases of Tasmania. An elaborate account of the yellow fever epidemics of Bermuda is from the pen of Dr. Smart, R.N., Deputy Inspector-General of Hospitals. Some curious notices of the epidemics of 1719-20 and 1759 in Peru, also of the Mexican hæmorrhagic disease of 1736 and 1855 are communicated by Dr. Archibald Smith. The Director-General of the Navy Department, Dr. Bryson, furnishes some curious data respecting an epidemic of pleuro-pneumonia in the Mediterranean fleet. A highly suggestive paper on the influence of pandemic causes in the production of fever is from the pen of Dr. Lawson, Deputy Inspector-General of Army Hospitals. Dr. Gavin Milroy, the President of the Society, contributes some notes on the epidemic and other diseases of the natives of India; and Dr. Bell, of Brooklyn, New York (U.S.), treats of the causes, malignancy, and persistence of fever aboard ship.

A brilliant and very hopeful paper by Dr. B. W. Richardson, on the position and prospects of Epidemiological Science, will command attention.

A report of the Smallpox and Vaccination Committee on the amendment of the Vaccination Laws; and a report of the Council on certain questions submitted to it by Dr. Farre concerning the classification of Epidemic Diseases, form appendices to the work. The former report is an admirable summary of the modifications required in the regulations concerning vaccination; and the latter report touches a question of growing public interest.

VI.—*Allgemeine pathologische Anatomie*. Von Dr. JOSEF ENGEL, Professor an der K. K. Med. Chir. Josefs Akademie in Wien. 8vo, pp. 560. Wien: W. Braumüller. 1865.

General Pathological Anatomy. By Dr. JOSEF ENGEL, Professor in the Royal Med. Chir. Academy of Vienna.

The volume before us is the first part of a more extensive work designed by Professor Engel as an aid to those who attend his lectures. It treats of a large number of topics without illustrations, but with much conciseness, so that it is difficult to give any adequate notion of its style or merits. Inflammation, with its various exudations and results, alterations of secretion, changes in the blood and

lymph, extravasation of blood, atrophy, hypertrophy, degeneration, other changes of nutrition, changes of colour, new formations, healing after injuries, parasites, and fetal malformations, are the chief heads under which its subject-matter is arranged. There are very few questions referrible to these heads on which some information is not given; but usually with great brevity. As a specimen of its style, we extract entire the article upon syphiloma, selecting it on account of the great interest attaching to the subject at the present moment:—

“Syphiloma may be here considered, because, like tubercle and sarcoma, it scarcely goes beyond the very commencement of organic formation, and because, like them, it is essentially composed of nuclear or cell-like elements. Between the three there is no other known resemblance, and their juxtaposition shows clearly with what small propriety anatomical structure is made the basis of the classification.

“Syphiloma appears in the form of nodules, from the size of a pea to that of a walnut or a hen's egg, round or variously shaped, sometimes sharply and sometimes obscurely defined, varying in consistence from that of stiff jelly to that of soft cartilage, and in colour, according to their structure, from white, or whitish, to yellowish. They exhibit a homogeneous section, and appear structureless to the naked eye. The same substance is found as a heterogeneous film, more or less thick, upon the surface of syphilitic ulcers, or in the form of granulations, or infiltrated among soft tissues. The base of syphilitic ulcers of bone is often covered by this formation.

“Microscopic examination of syphiloma reveals a thick connecting tissue, sometimes regular, sometimes felted, in the interstices of which there are collections of roundish nuclei, and sparsely-distributed cells. These nuclei and cells often lie in an amorphous stroma.

“The formation of syphiloma takes place in the interstitial tissue, or, in glands, from the inter-acinous connecting tissue; this tissue increasing at the expense of the other proper elements of the organ, which either disappear completely, or undergo great changes, and remain imbedded in the syphiloma in the form of larger or smaller bands, granules or islands, visible to the naked eye. In this way disappear all but small remnants of gland cells, and capillary vessels, in the localities in which syphiloma is developed.

“Syphiloma undergoes the ordinary metamorphoses. In a syphiloma that contains a large proportion of nuclear or cell-like formations, these may become tuberculous. The disease then forms nodules of various size and form, possessing the hardness, yellowish colour, and homogeneous fatty lustre of yellow tubercle; and either invested by a hard, thick, fibrous tissue, or inter-penetrated by its bands. Where the cells and nuclei are inconsiderable in quantity, the syphiloma becomes converted into a tough cicatrix, seldom sharply bounded, hard, white, or yellowish white, interspersed with yellowish granules, and surrounded by a considerable loss of tissue, or fatty degeneration of parenchyma cells, or, if not superficial, by contraction of the neighbouring parts. Sometimes the hard, cicatricial substance contains small collections of chalky matter. In old syphiloma, it is unusual to find cells, but only aggregations of differently shaped, small, shining corpuscles (tubercle corpuscles).

“Syphiloma is described, according to the size of the masses, as nodular or military; according to its limitation, as diffuse or limited; and the softer masses are also described as syphilitic gummy tumour. E. Wagner considers the product of interstitial hepatitis to be a syphiloma.

“There is scarcely any texture or organ in which syphiloma is not found.

It invades the skin, the mucous membranes (chiefly of the throat, larynx, and genitals), the periosteum, the muscles, and the heart. Among glands, it is found in the liver, lungs, spleen, kidneys, and testes. Its metamorphoses, and also its shape, size, and other peculiarities, vary with its locality. The tissues in which it appears are mostly otherwise diseased; as the bones, by sclerosis, osseous ulcer, or osteoporosis; the liver, by fatty or speckled degeneration; the spleen and kidneys, by speckled degeneration; the skin, by condylomata, eruptions, and ulcers; the mucous membranes, by mucous tubercle, ulcerations leading to hard, contractile cicatrices, and chronic blennorrhœa; and all organs, in fine, by some ulcerative or formative inflammation; so that it is associated with all the various products and chronic changes that follow from venereal disease."

VII.—*The Principles and Practice of Medicine; designed chiefly for Students of Indian Medical Colleges.* By JOHN PEET, M.D., F.R.C.P., &c. London: J. Churchill and Sons. Bombay: Thacker, Vining, and Co. 8vo, pp. 560. 1864.

Many important and interesting treatises have been written on the diseases of India, but this is the first instance of a work, coming to us from that quarter of the globe, which is so comprehensive in its scope as to embrace the whole field of medicine. Although somewhat elementary in character, probably from its being chiefly designed for students of Indian medical colleges, the book is pleasantly written in a clear, lucid style, and will be found, as regards the diseases which are most prevalent in India, a good introduction to Sir Ranald Martin's important work, and to Dr. Morehead's excellent "*Clinical Researches on Disease in India.*" It will help to dispel the erroneous notion at one time entertained, that the diseases of India are totally different from those with which medical men are familiar in colder or more temperate regions, and that, like the produce of the soil, they are of indigenous growth, differing in their etiology, their mode of development, and requiring on this account different modes of treatment and different remedies. More careful observation has now shown that many of the complaints said to be peculiar to India are well known in Europe, and that the mistake has simply arisen from the too easy adoption of strange and uncouth terms for diseases already known in Europe under different names. We have instances of this in *Beri-beri*, a name given in India to cases of acute general dropsy supervening after exposure to cold or wet; and in *Barbiers*, which seems, after all, to be nothing more than ordinary acute myelitis, an affection which is of extremely frequent occurrence on the Malabar coast, and which, probably owing to the sudden transition from excessively hot days to cool and almost cold nights, occurs in those latitudes at certain seasons of the year.

In Dr. Peet's work, prominence is naturally given to those diseases which are of greatest frequency in India; and in his chapters on dysentery, hepatitis and abscesses of the liver, tetanus, leprosy, &c., the author speaks with the authority of one who has seen a good

deal of Indian practice. We regret to find, however, that he does not concur in the opinion so ably advocated (not very long ago) by Sir Ranald Martin, as to the propriety of opening abscesses of the liver. True, one may well hesitate before pushing a long trocar into the liver in order to *search* for an abscess, however innocuous the practice may have been stated to be; but when the *existence* of an hepatic abscess has been clearly *ascertained*, we are far from agreeing with Dr. Peet's statement, that "an artificial opening increases the patient's sufferings, and hastens his death." The chapter on leprosy (a subject which has recently engaged the attention of Government and the London College of Physicians) contains some valuable information, and the author confirms the generally received opinion that the disease is *not* contagious. With regard to the important question of the communicability of cholera, Dr. Peet states that "there is unquestionable evidence that it *may* be conveyed from one person to another, although it is probable that the contagion is not very violent, or that most persons are less susceptible of its action than that of most other contagious affections; hence it follows that the spread of cholera by human intercourse is very limited."

In conclusion, we gladly share the author's hope that this work will be found useful not only to those who are still *in statu pupillari*, but also to those medical officers who are for the first time entering medical service in India.

VIII.—*A Treatise on Hygiene, with special reference to Military Service.* By William A. Hammond, M.D., Surgeon General of the United States Army. 8vo, pp. 604. Philadelphia. 1863.

For a long time a treatise on military hygiene in the vernacular has been a desideratum. The armies of no nation have suffered more than those of England from the neglect of hygiene. No nation has been taught by more bitter or oft-repeated experience than ourselves the need of a systematic training of officers, whether military or medical, in the principles and practice of hygiene. No nation has shown greater aptitude or energy, at the moment, in profiting by such experience; as witness the history of English military hygiene in the Crimean war. But although we can now boast of efforts for the sanitary reform of our armies, unrivalled in magnitude, conception, and success; although we can point to vast official documents, which almost exhaust practical and theoretical knowledge, on military hygiene, and form an unequalled repertory of all things that most immediately relate to the subject; although hygiene has become a necessary part of the officer's training, whether he be a combatant, engineer, or doctor; and, finally, although we have a special school for indoctrinating medical men into the art and mystery of preserving soldiers in health, and treating them when sick, and special professors for teaching such art and mystery

—until the publication of Dr. Hammond's work, no systematic treatise on military hygiene existed in the English language. Since the appearance of this work, however, the reproach has been removed by Professor Parkes.

The Americans, at the outbreak of the sad strife which has recently terminated in the United States, took early example from our defects. Holding in remembrance the terrible winter of 1854-55 before Sebastopol, they profited by our experience. A Sanitary Commission was formed at the beginning of the civil war. This Commission, in the first instance, prepared brief digests of all those facts which would best guide the medical officers attached to troops, as well as the combatant officers and men, in preserving health in camp and during a campaign. The most important of the digests were prepared from the English official documents relating to the health of the British Army during the Crimean war. These valuable and handy papers were distributed broadcast among the Federal armies; and sanitary inspectors, appointed by the Commission, gave practical effect to the advice urged in the printed documents. Early in the war, also, Professor John Ordovnaux, of Columbian College, New York, published a capital little handy-book, entitled *Hints on the Preservation of Health in Armies; for the Use of Volunteer Officers and Soldiers*. And now the Surgeon-General of the Federal forces has published a systematic treatise on the subject.

This book is ably written, largely illustrated, abounds in facts, and well fulfils the object for which it was intended. For the young military medical officer it will prove an excellent guide, and it is equally suitable for combatant officers. It may also be studied with profit by all medical students and medical men whatever.

The work is divided into three sections, the first being devoted to the examination of recruits; the second to the agents inherent in the organism which affects the hygienic condition of man—*e.g.*, race, temperament, idiosyncrasy, age, &c.; and the third to agents external to the organism which act upon the health of man. The chapters of the latter section devoted to hospitals, camps, and food, are admirable; there is a defect, perhaps, in detail in reference to camps and camp-life. In subsequent editions it would be well to enter more fully into the hygiene of camp-life, and particularly into the subject of cooking. The conclusions of the English Army Sanitary Commissions are largely adopted by Dr. Hammond. It is impossible, with the space at our command, to do justice to this work point by point. We shall, therefore, briefly cull from it a few facts relating to the Federal forces at the time when the book was published.

Dr. Hammond gives an interesting table, showing the great stature of American as compared with English and French soldiers. Of 1000 men in the British army, there were but 65 of six feet and over, and in the French army but 4; while of 1800 recruits for the United States army, 251 were six feet and over in height, or somewhat more than 133 per 1000.

"The American soldier," Dr. Hammond tells us, "is better fed than any other in the world; the ration, as established by law, consists of:—

	lbs.	ozs.
Bread or flour	1	6
Fresh or salt beef	1	4
or		
Pork or bacon	0	12
Potatoes (three times a week)	1	0
Rice	0	1·6
Coffee	0	1·6
or		
Tea	0	0·24
Sugar	0	2·4
Beans	0	0·64 gill.
Vinegar	0	0·32 „
Salt	0	0·16 „

“In addition to the above, one pound of sperm candles, or one and a half pounds of adamantine candles, or one and a half pounds of tallow candles, and four pounds of soap, are issued to each 100 rations. Pepper has recently been added to the ration.

“Extra issues are made of pickles and fruits, saurkraut, and other vegetables, whenever, in the opinion of the medical officers, they are necessary to the health of the troops; and one gill of whiskey is allowed in cases of excessive exposure and fatigue.

“Whenever it is practicable for the troops to bake their own bread, flour is issued. Twenty-two ounces of flour, if properly baked, will make about thirty ounces of bread. The surplus flour is resold to government at the cost price, and thus a fund is formed by each company which is used for the purchase of such additional articles of food or comfort as may be desired. In time of peace, company gardens are cultivated at every military fort, so that it scarcely ever happens that there is any deficiency of food, either in quantity or quality. Fresh meat is issued as often as the commanding officer may direct—generally about four times a week.

“Since the commencement of the present rebellion, the armies of the United States have been fed as no other armies ever have been fed in time of war. This is proven by the healthy condition of the troops, wherever the influence of a bad climate has not been in force. Scurvy, for instance, one of the first diseases to make its appearance when the food is of inferior quality, has prevailed to so slight an extent that the occurrence of an occasional case excites attention. When we compare the condition of our troops in this respect, with that of other nations during extensive warlike operations, we may well congratulate ourselves on the difference.”

IX.—*A Manual of Practical Hygiene, prepared especially for use in the Medical Service of the Army.* By EDMUND A. PARKES, M.D., F.R.S., Professor of Military Hygiene in the Army Medical School; Member of the Medical Council of General Education; Examiner in Medicine in the University of London, &c. Churchill. 1864. 8vo, pp. 612.

If to Dr. Hammond belongs the honour of publishing the first systematic treatise on hygiene in the English language, to Dr. Parkes appertains the merit of publishing the most useful text-book on the subject. As a manual of military hygiene, Dr. Parkes's book is perfect; as a guide to civil hygiene it is the best we possess. Although prepared especially for military service, Dr. Parkes has kept in view the needs of civil practitioners. He has discussed all the general questions of hygiene so broadly, that in this respect the book is adapted to all classes of persons, lay or professional, who take an active interest in, or have to deal practically with, health questions, public or private.

The book is divided into two parts. In the first, the chief subjects of hygiene are discussed, and illustrated by examples drawn from army-life, some topics important to medical officers to know, such as meteorology and statistics, being included; in the second, the service of the soldier is more particularly described. Dr. Parkes has "thought constantly on what would be useful to army surgeons, who are often far from all books or possibility of reference."

In a work of this class, abounding in details on numerous subjects, and where condensation of material is aimed at, it is not easy to separate one portion from another so as to give a just idea of the author's style and merits. It is best, perhaps, to seek to convey a general notion of the contents and arrangement of the work.

The first chapter treats of *water*, the quantity required by the healthy and the sick, its quality and composition from different sources, the mode of its examination—physical, microscopical, and chemical—its purification, and the consequences of an insufficient or impure supply.

The second chapter discusses *air*, the quantity required by the sick and well, its composition, the impurities contained, or apt to be contained, in it, the septic condition of the atmosphere, the purification of air by solids, liquids, and gases, and the effect of disinfectants on the specific diseases.

The third chapter treats of *ventilation*, dealing with the vexed subject in a way to be commended to every medical man.

The fourth chapter is devoted to the *examination of the air and the sufficiency of ventilation*.

The fifth chapter deals with *food*, in health and disease, its digestibility and variety. It deals also with the soldier's ration, and with diseases connected with food.

The sixth chapter discusses the quality, choice, and cooking of food, and diseases arising from altered quantity. The fault of this chapter is the insufficiency of the paragraphs relating to cooking.

This is true also of Dr. Parkes's remarks on cooking in the field. The subject is one of the highest importance, and one with which the military or civil medical man should be thoroughly familiar. Dr. Parkes might have taken some valuable hints both from the French, and particularly the Turkish soldiers, with much advantage to his readers. Beverages and condiments, soils, habitations, including hospitals, sewerage, and the method of removing sewage matter, warming, exercise, and physical training, clothing, and the soldier's dress and accoutrements occupy the seventh, eighth, ninth, tenth, eleventh, twelfth, thirteenth, and fourteenth chapters respectively.

Meteorology, and instruments used in the army, are considered in the fifteenth chapter; climate is treated of in the sixteenth, and the prevention of some of the army diseases, in the seventeenth.

The observations in the last-named chapter on the prevention of venereal diseases—the great bane of the English soldier—are peculiarly good. Dr. Parkes justly remarks that, in reference to restricting the opportunities of infection in the home stations, that meaningless phrase, “an interference with the liberty of the subject,” should disappear.

The eighteenth chapter is devoted to the disposal of the dead; the nineteenth to individual hygienic management; and the twentieth to statistics.

The second book concerns the service of the soldier. It is considered in relation to the recruit, to home and foreign service on board ship and in war. The chapter on foreign service contains a summary of the climate and sanitary condition of the different stations occupied by British troops abroad, including India.

This brief outline of the contents of Dr. Parkes's manual will indicate its wide scope, and its immediate bearing upon the requirements of both civil and military medical men.

X.—*Des Maladies Mentales et des Asiles d'Aliénés : Leçons Cliniques et Considérations Générales.* Par le Dr. T. P. FALRET.

On Mental Diseases and Lunatic Asylums. By Dr. T. P. FALRET. Paris. 1864. T. B. Baillière.

In these days of book manufacture, when men but little acquainted with a subject personally still contrive to compile thick volumes upon it, it is refreshing to turn to a book like the one now lying on the table before us. The work of a veteran physician, a pupil of Pinel and Esquirol, it embodies the result of many years' experience, and is the fruit of long and patient research. It is not a systematic treatise on mental diseases, but consists of separate, unconnected chapters, each subject being treated with great skill, and in a truly clinical and practical manner. To these chapters is prefixed a long introduction (perhaps the most important portion of the book), in which the author summarizes his views regarding the mode of studying mental diseases, and the proper treatment adapted to them.

Physicians who make mental diseases their special study divide themselves into two great classes: the one believing that pathological

anatomy can alone give us the clue to the elucidation of the phenomena of mental aberration under its various forms, and therefore expecting, in all cases, to find anatomical lesions that will explain those phenomena; the other, on the contrary, rejecting pathological anatomy, and trusting to the application of metaphysical doctrines to the study of mental diseases, as alone capable of giving us the key to the solution of the complex problems arising from such diseases. The one may be termed the anatomical, or somatic, the other, the psychological school. But by the side of these two schools, much too exclusive in their views, there has arisen a third, which is a mixture of the two former, which has hopes, by the combination of pathological research and the application of metaphysical science, of being enabled gradually to unravel the mysteries of mental diseases. It is to this third school that Dr. Falret belongs. But it was not at once and from the beginning that he came to adopt these views. At the commencement of his career, he felt, like many others, the influence of the pathological movement which was then beginning to renovate and almost revolutionize every department of medicine; and, although a pupil of Pinel and Esquirol, he fondly believed in the power of anatomy to solve the intricate problems of mental diseases. But as his knowledge increased, or, rather, as experience came to him, he found how inefficient anatomy was to explain the infinite variety, and the multiplicity of the forms, of delirium. He then tried the principles of the metaphysical school, but, as he says himself, he found that by so doing—

“He destroyed all that essentially constitutes a disease, namely, the grouping of connected symptoms and their order of sequence; that he did away with all notion of progress and co-ordination of phenomena, ignoring the disease and viewing the symptom only, and ignoring the complex to view the isolated symptom alone.”

After a long and laborious career, he now makes the following profession of faith:—

“I belong to the anatomical school, since I admit the existence of some organic lesion in all cases of mental disease; but I belong also to the psychological school since Hook, to a careful and minute observation of psychical phenomena, and to the production of delirium by delirium as the chief basis of the etiology, symptomatology, and therapeutics of mental diseases. . . . The primary organic modification, unknown in its essence, but discoverable in its effects, is the real cause of mental diseases, and first gives rise to what I term ‘*the aptitude for delirium*.’ But delirium, once produced in this way, next develops itself after laws which are special to itself. . . . Ideas and feelings, which originate in the human brain, both in health and disease, in consequence of the combined action of the soul and body, give rise to others in a successive series. This is what I have called the theory of the ‘*psychical resultant*,’ because in the incessant action of the mind upon itself, the first result produced, or the first *resultant* causes new effects, secondary, tertiary, etc. This mysterious faculty, which multiplies indefinitely the number of phenomena produced, can alone explain, in our opinion, the complexity and diversity of psychical phenomena.”

After this statement of the author's views, and his theory of the production of delirium by delirium, we can easily understand his rejection of monomania from the nosology of mental diseases, and his emphatic declaration that he never met with a pure and genuine case.

of monomania, either in his hospital or private practice. "Nothing," he says—

"Can be more false or more contrary to observation than the fragmentary division of the human soul into a certain number of distinct forces, capable of isolated action, and consequently apt to be separately affected. All the faculties of man are linked together in their action, and it is only by an abstraction to facilitate study that the various modes of human activity may be regarded as special forces, whilst they are only varying aspects of one and the same principle, indivisible in its unity. Theory, therefore, as well as observation, disproves, in my opinion, the philosophical principle on which is based the existence of monomania." (p. 432.)

In February, 1851, the Académie Impériale de Médecine of Paris was engaged in a discussion *à propos* of a paper read by Professor Trousseau, on "Apoplecticiform Cerebral Congestion." Dr. Falret took part in the discussion, and espoused Professor Trousseau's cause, concurring in his statement that apoplecticiform cerebral congestion may be one of the manifestations of epilepsy. In the work before us, Dr. Falret consecrates a chapter to this important question, and takes great pains to prove that Professor Trousseau was wrong in denying the existence of such congestions in connexion with other cerebral diseases besides epilepsy. Now, whether Professor Trousseau's views were misapprehended at first, or whether they have been modified since, we cannot tell; but in his recently published *Clinique Médicale*, the learned Professor fully concurs in the views expressed by Dr. Falret, and endeavours to establish the fact that, in most cases, apoplecticiform cerebral congestion is associated with epilepsy (idiopathic or symptomatic), or with eclampsia, that is, with convulsions supervening in the course of the general paralysis of the insane, or chronic lead poisoning, or with convulsions due to the presence of worms, &c.

On the important subject of treatment, the book, as might be expected from the great experience of the author, contains most valuable suggestions. Although medicinal as well as moral treatment is recommended, it is plain, however, that considerably greater importance is attached to moral treatment for the cure of insanity. The author speaks in favour of large asylums for the collective treatment of the insane, and recommends the system of *cottages* for special cases only. He dwells on the immense advantages arising from a life in common, and the association of patients in general sitting-rooms, whereby the tendency to self-isolation and concentration of ideas, so frequent in insanity, is obviated in a great measure, and the chain of insane delusions is interrupted or broken. But what does as much credit to the benevolence of the man as to the talent of the physician, is the warm recommendation of a system by which convalescents on leaving an asylum are visited at home and procured employment. A society was originated in Paris by Dr. Falret, with this object in view, under the name of "Société de Patronage des Aliénés Convalescents," and it is gratifying to learn that their humane efforts have met with the success which they deserved.

In conclusion, we strongly recommend Dr. Falret's work, as one which is full of excellent practical suggestions, and which is written in a truly scientific, as well as in a humane and Christian spirit.

XI.—*Clinical Observations on Functional Nervous Disorders.*

By C. HANDFIELD JONES, M.D, Cantab.; F.R.C.P., Physician to St. Mary's Hospital. London: Churchill. 8vo, pp. 576. 1865.

To analyse this large volume fully would be quite impossible; we therefore propose to make some general remarks on the style of its execution, and the tone of the author's views on disease.

We willingly accord to Dr. Handfield Jones the praise of industry, and the equally important merit of discerning, in some degree at least, the special signs of the times. He is aware that the vague expressions in which theories of functional nervous disorder have been for the most part clothed are unsatisfactory, and that it is necessary, if progress is to be made, that the classification of these complaints should be revised with the assistance of modern physiological information. He recognises, also, the need of increased precision in our ideas of the action of remedies in these disorders. We wish that we could say that the attempt which he has made to meet these wants was as successful as it is laudable; but this is hardly the case. Throughout the volume we frequently meet with passages in which the reader, finding himself with pleasure on the track of a valuable idea, is checked in a mortifying way by the author's failure to grasp his subject completely. We think it will be more useful, instead of applying to this book the general terms of moderate laudation to which it is undoubtedly entitled, but which its author would scarcely thank us for, to point out certain definite shortcomings, as examples of the faults of which this work cannot be acquitted.

First, with regard to the author's explanation of the conflicting theories on the subject of the so-called "inhibitory influence." Our readers will remember the general course which the controversy on this subject has taken,—how Pflüger, in the first place, imagined that a particular set of nerves was devoted to the especial and sole purpose of restraining muscular action; and how Lister subsequently overturned this doctrine by demonstrating that the very same nerves might be the channel of impressions which should either increase or diminish the contractions of muscles, and that in order to produce these differences it was not necessary to alter the *kind*, but only the *strength* of the impression; and finally, how the more recent researches of Moleschott have established the general correctness of the experiments of Lister. Dr. Jones, rightly enough we think, declines to assume, in the interpretation of these very interesting facts, that the amount of nervous "action," excited (in the nerve immediately impressed) will be directly proportionate to the violence of the impression. He is inclined to see, in the condition of the afferent nerve, which has received a powerful shock, a state of depression rather than of energetic action, and there is little doubt that he is right in this, and that the expressions used by Mr. Lister are objectionable. The word "disturbance" would have been more appropriate than "action." What Dr. Jones fails to see is that the state of the afferent nerve, when impressed by a *weak* electric (or other) *shock*, is also one of disturbance, rather than of action,

properly so-called, and that the quickening of the heart's movements thereby produced—*e.g.*, in Moleschott's experiments—is decidedly abnormal. An influence which causes the pulse of a rabbit to rise at once from 166 to 231, or that of a frog from 30 to 142, ought to be regarded as a disturbing, not a fortifying influence, at any rate in its direct effect. The result of Moleschott's experiments on the heart is plainly this: the vagi and the sympathetics are amenable to electric impressions, on the whole, in the same way. They are, in respect of the influence of shocks, *mere channels of disturbance*; their delicate and susceptible organization being very easily disturbed (especially that of the sympathetics). If we take the sympathetics (as being the more impressible) we can demonstrate by a simple experiment that the effect of *slight lesion* of this nerve is an immediate *increase in the frequency* of the heart's movements, while that of a *very serious lesion* is immediate *cardiac palsy*. It is difficult to see how the term *stimulation* can be applied to such impressions upon a nerve as were made in the experiments of Moleschott, by which the heart's action was quickened, and which consisted in electrification, friction, stretching, or pressure of the naked nerve, or in the application of salt-water, or of desiccating heat to it, while the term *lesion* is to be reserved for the severer electrical or other impressions which are sufficient to bring the heart to a standstill. The distinction seems quite arbitrary, and precisely the same remark, as it appears to us, may be applied to the experiments of Lister; for surely the passage of a series even of weak electrical shocks through the pneumogastric nerve of a healthy animal amounts, *pro tanto*, to a lesion.

We think, therefore, that Dr. Jones has only grasped a half-truth. He objects, as we think very properly, to attribute "energetic action" to a nerve which has been overwhelmed with a series of powerful electric shocks; but, misled by the apparent opposition between abnormally hurried action of the heart and total cessation of its movements, he opposes the cause of the one to the cause of the other—a position for which we can perceive no reasonable grounds. Both the one and the other interfere with and diminish the normal regulating action of the nerve upon the motile organ, though in different degrees. We dwell thus upon what may seem merely a question of physiology, and apart from the chief practical purpose of the book, because we think it indicates an unconscious tendency of the author's mind, which is influential in the formation of his theories of functional nervous disorder, and not for good—a tendency, namely, to cultivate abstractions and entities. With him, *vis nervosa* is still an abstract something which can be "stimulated;" and so, instead of breaking clear of the dull round of vague generalities, which is the curse of "nervous" books, he repeats much of this tiresome phraseology, and thereby greatly hampers his own efforts to improve our knowledge of his most important subject.

Of the tendency to embody what are, after all, mere phrases into entities endowed with active powers, we shall give one more example from the author's pages. In the chapter on "Cardiac Neuroses" is included a short treatise on angina pectoris, the very

first sentence of which is enough to give anyone who has bestowed a thought on the needs of nosological science a cold turn. Dr. Jones says—"I consider it (angina) to be *au fond* a neuralgia." Now, if there be one fact in pathology which more than another we should be inclined to believe indubitable, it is that angina consists in an arrest of the heart's movements, which is always attended by pain, but which may depend, ultimately, on either of several organic changes. The pain, we take it, is the mere mental interpretation of the intense and sudden embarrassment of an organ whose movements are immediately necessary to life, the nervous system meanwhile maintaining much more nearly its normal vital conditions and sensations, not being blunted, as would be the case in the cardiac embarrassment towards the fatal close of a thoracic inflammation, a fever, &c. Except in a secondary sense, and to a limited degree, the pain of angina has no right to be considered lethal, while the disease itself is eminently so. Dr. Jones is, doubtless, correct in describing the anginal pain as neuralgic in character; but he errs in constituting cardiac neuralgia into an entity, endowed with the power of producing the symptoms of angina pectoris. To put the matter thus is still further to confuse an already most difficult subject.

We might bring forward similar objections to the chapter on remedies, where, again, a good idea is spoiled by a too theoretic and fanciful carrying out. But we have no mind for further fault-finding; and we are bound to say, in taking leave of the book, that it contains much matter which is really of value. As a transition-stage towards improvement, it demands the attention of all those who are struggling to give a more scientific tone to practical medicine; and were it the work of a younger and a less deservedly esteemed teacher than Dr. Handfield Jones, it would satisfy us better than it does. We trust that future editions will show a material improvement in the matter of precision of ideas.

XII.—*Practical and Pathological Researches on the various forms of Paralysis.* By EDWARD MERYON, M.D., Fellow of the Royal College of Physicians of England, late Lecturer on Comparative Anatomy at St. Thomas's Hospital, &c. John Churchill and Sons. London, 1864. 8vo, pp. 215.

Diseases of the nervous system have of late years attracted considerable attention, both here and abroad. There are so very few fields, in the wide range of medical science, that have not been so thoroughly explored as to leave newcomers more than a faint hope of ever doing anything original, that the nervous system, viewed under both its physiological and pathological aspects, offers peculiar charms to active and ambitious minds. Rich and inexhaustible mines, or at least mines unexhausted as yet, are there to allure and encourage; and it is no matter of surprise, therefore, that so many

earnest labourers have been found to enlist in the work, and put their shoulders to the wheel.

Dr. Meryon has evidently felt this wide-spread and growing influence, and the result has been his recent publication of a work on Paralysis. The book does not aim at originality, nor is it made the vehicle of any pet theories or methods of treatment. It merely attempts a comprehensive, although somewhat of a bird's-eye view, of the wide subject of paralysis, aided by the new lights thrown upon it by the numerous recent discoveries which have almost revolutionized this branch of medical science and practice. But this was no easy task—not, indeed, from dearth of materials, for these were abundant and at hand; but because so much has lately been done to shake and modify, and in some cases entirely to upset, our former theories and notions respecting nervous physiology and pathology, that great tact and judgment were necessary for making a proper selection, and arriving at conclusions which might not clash with and stultify one another.

The work is divided into six chapters or parts. The first of these contains an interesting summary of the minute structure of the nervous centres, chiefly after the researches of Lockhart Clarke, and Schroeder van der Kolk. The author also corroborates Stilling's statement as to the existence of longitudinal nerve-fibres in the anterior cornua. The physiology of these nervous centres is merely touched upon, but Dr. Meryon evidently belongs to Longet's school in regarding the posterior columns as the conductors of sensitive impressions to the brain, and the anterior as the conductors of the orders of the will to the motor nerves. Hence, when treating of myelitis, he gives as diagnostic signs of the portion of the cord diseased, paralysis of motion when the anterior columns chiefly are affected, but of sensation, if the lesion exist in the posterior columns; and, he adds, if a careful analysis be made of the several cases in which the grey substance of the cord has been implicated, it will be found that the function of reflex action has been deranged (p. 30).

But that part of the statement which relates to sensation is evidently in direct contradiction to Dr. Meryon's acknowledgment that the experiments of Dr. Brown-Séguard have proved that the decussation of the nerves of sensation takes place in the spinal cord (p. 5). It is not, however, the latero-transverse fibres of the grey matter, as Dr. Meryon suggests, which are the conductors of the impressions of sensation from one side of the body to the opposite side of the spinal cord. The experiments of Dr. Brown-Séguard, just alluded to, have proved, on the contrary, that the transverse-fibres are employed for reflex action, and hardly for the transmission of sensitive impressions; this latter function devolving on the ascending and descending fibres, which, from the posterior roots, travel for a short distance along the posterior columns of the cord, and then enter the grey matter. (See Brown-Séguard's *Lectures on the Nervous System*, p. 36.)

In the second chapter, after a rapid review of the various forms of paralysis depending on hydrorachis, spinal congestion, and spinal meningitis, with effusion, the author dwells at greater length on the

important subject of myelitis. He shows how the symptoms differ according to the height at which the cord is diseased; and he endorses Brown-Séquard's assertion, that when the disease is confined to the lumbar enlargement, convulsive movements occur at an early period, and cease, *pari passu*, with the disorganization of the cord. The persistence of spasms in the paralyzed legs is, therefore, indicative of myelitis above the dorso-lumbar enlargement, and thus becomes an important diagnostic sign.

With regard to treatment, without attempting to explain the mode of operation of ergot of rye, Dr. Meryon believes "that we have every encouragement to depend on it in chronic myelitis, especially when the pelvic viscera are implicated in the paralyzing influence. He has given the æthereal tincture in doses of from ten to twenty drops twice or three times a day, and certainly with advantage; but he has not experienced the relief to reflex convulsions from it which he had been led to expect, and has therefore trusted to prussic acid, digitalis, and belladonna for that purpose.

It is only after the subsidence of all inflammatory action that he ventures on the administration of strychnine, and he even recommends that the internal administration of this energetic stimulant of the spinal cord be preceded by its external use, together with other stimulants in the form of embrocations over the spine. Electricity he regards as a therapeutic agent of great value, and is convinced, from experience, that the continuous current is just as efficacious as the inducted or intermittent current. But whether galvanism or electro-magnetism be employed, no high degree of tension is required for the restoration of muscular power. On the contrary, on the strength of Pflüger's experiments, who found that a very powerful current suspends the action, and perhaps even causes the death of the nerve-molecules, the author suspects that the favourable course of many a case has been retarded by the employment of strong currents.

The third chapter treats of the vast and important subject of paralysis from affections of the brain. The author is one of those who firmly believe that the brain is a series or congeries of organs, and expresses the opinion that "if every nervous fibre could be traced to its absolute terminus or origin in the brain, it would probably be found that every part of the body has its representative in a determined part of the brain, so that any lesion of that part must affect the function of the organ from which it receives or to which it sends nerves."

Those rare and anomalous cases in which hemiplegia occurs on the same side as that of the lesion are specially noticed. Dr. Brown-Séquard was the first to notice that this anomaly was apt to occur in cases of a tumour developed in the interpeduncular space behind the mamillary bodies, and regarded it as owing to reflex action. The author plausibly enough, however, suggests that it may be due to pressure on the upper portion of the non-decussating fibres of the medulla oblongata, which may perhaps be regarded as the direct channels of communication between the two crura cerebri and the corresponding sides of the spinal cord. An interesting case of this

anomalous form of paralysis, which fell under the observation of Dr. Ogle, is mentioned by the author. The post-mortem examination disclosed an aneurism of the left anterior cerebellar artery, so placed as to implicate the superficial part of the pons Varolii.

Although paralysis is not necessarily an attendant on diseases of the cerebellum, the author reviews this interesting subject, and suggests that at some future period it will perhaps be shown that the "central lobe of the cerebellum has some physical connexion with the genital organs; that the nerve-fibres which pass to the corpora quadrigemina are implicated in cerebellar disease when amaurosis results therefrom; that those portions of the lateral lobes which send transverse fibres to the crura cerebelli to interlace with the fibres of the corpora pyramidalia, are involved in disease when there is loss of co-ordination of muscular motion; and that that part of the cerebellum which overlies the fourth ventricle, and which is in the immediate vicinity of the nuclei of the eighth pair of nerves, is affected when obstinate vomiting is a prominent feature of the disease" (p. 145).

The forms of paralysis dependent on blood-poisoning, are next grouped together in a separate chapter, and the author goes fully into the comparatively new subject of diphtheritic paralysis. The injurious effects of that protean disease, syphilis, on the nervous system, are also described, but the author is obliged to acknowledge the unsatisfactory character of the diagnostic signs we as yet possess. He believes that paralysis may occur without any appreciable lesion of the nervous tissue, from the mere circulation in the blood of the syphilitic virus; in which case it generally appears at an early stage of the disease. When it is dependent on exudation of plastic lymph, it may be a year, or 2 years, or 5, or 10, or even 20 before it supervenes.

Reflex paralysis has a whole chapter devoted to its consideration; but the author merely reproduces Dr. Brown-Séquard's arguments, and concurs with them entirely.

The book concludes with a sketch of what the author terms "progressive forms of paralysis." Under this head he groups together the general paralysis of the insane, the ataxie locomotive progressive of the French, and what he terms paralysis from granular degeneration of the voluntary muscles. This account of general paralysis, borrowed from Calmeil, relates only to that which occurs in the insane; he makes no mention of a similar form which occurs independently of any previous stage of mental aberration or excitement, and of which a good account is still a desideratum.

The section on progressive locomotor ataxy, or Duchenne's disease, which the author calls "progressive ataxia," is an incomplete *résumé* of Professor Trousseau's Clinical Lectures on the subject. As to the nervous nature of the complaint, there cannot be the shadow of a doubt. Numerous post-mortem examinations, many of them recorded in Professor Trousseau's second lecture, have now established the fact, that gelatiniform degeneration of the spinal cord is the rule in such cases, attended with destruction of the nerve-fibres and cells, increase of the neuroglia, and development of the so-called

amyloid corpuscles. That the disease is nothing more than the *tabes dorsalis* of the ancients is scarcely doubtful either, although the fact of the author describing the two diseases separately, shows that he believes them to be two separate and distinct morbid entities. Romberg's account of *tabes*, however, is a masterly picture of Duchenne's disease, in which but one finishing touch is wanting to make the picture complete, namely, the amount of *real* muscular strength, as contrasting with the *apparent* paralysis of motor power, a most important and typical character of ataxy, which Dr. Duchenne, de Boulogne had the credit of discovering.

The third form of progressive paralysis is that which depends on granular degeneration of the muscular tissue. The section which treats of it is the most original of the whole book. This peculiar complaint is characterized by the absence of any symptoms of centric disturbance, the only phenomenon of disease being gradual diminution of muscular power. This, according to the author's experience, begins generally in the *lower extremities*, and extends by slow degrees to all the voluntary muscles. According to Cruveilhier, Aran, Trousseau, and others, however, the disease much more frequently attacks the muscles of one of the upper limbs first, and especially the muscles of the ball of the thumb and the inter-ossei. The author shows how it is apt to run in families, and believes it to be due to a diminished supply of arterial blood, and defective nutrition in the diseased muscles, which are well-nigh bloodless, just as is the case with bones in rickets. In proof of the analogy which he traces between the two diseases, he quotes the case of a boy, aged eight, who twice broke his thigh by simply falling on the floor. Anatomically, the disease is characterized by a disruption of the striped muscular fibres, *without any abundance of oil globules*. In many places the fibres are completely destroyed, the sarcolemma is diffused about in the form of granular matter, whilst the sarcolemma is broken up and destroyed. Dr. Meryon denies that the anterior roots of the spinal nerves, or any other portion of the nervous system, are ever diseased in such cases.

With regard to treatment, the author suggests, that as arsenic has a peculiar preservative action on dead animal tissues, it might also be the means of preserving living fibres from a disorganization which appears to result from malnutrition. In one case only of this usually intractable disease had the remedy been used, and the progress of the granular degeneration appeared to have been arrested.

XIII.—*De l'influence Pathogénique des Maladies Pulmonaires sur le cœur droit.* Dr. XAVIER GOURAUD. Paris, 1865
L. Leclerc.

On the Pathogenetic Influence of Pulmonary Maladies on the right side of the Heart.

This interesting memoir, of about 200 pages, treats of a very important question in pathology and in clinical medicine, namely,

the influence of pulmonary affections on the causation of diseases of the right heart. This influence has long ago been observed, and is universally accepted in the case of chronic pulmonary diseases. But the author goes further, and ascribes the same influence to acute diseases of the lungs; and he gives cases observed by himself, as well as borrowed from eminent authors, in support of his views. An ingenious statement of his deserves particular mention. After having tried to show that acute pneumonia may, in some cases, produce passive dilatation of the right chambers of the heart, and sequential tricuspid regurgitation, he ingeniously suggests that this series of phenomena may serve to explain the frequent coexistence of icterus in pneumonia. The jaundice, according to him, would be the result of the above changes, through distension of the vena portæ and vena cava inferior, and congestion of the liver. Thus might be explained the occurrence (in those rare cases of which instances may be found in special treatises, as in the remarkable monograph of Grisolle on pneumonia) of jaundice supervening in the course of pneumonia, affecting not the *right*, but the *left* lung; nay, more, the *apex* of the left lung.

XIV.—*An Inquiry into the Relative Frequency, the Duration, and Cause of Diseases of the Skin, as deduced from the Observation of 1000 Consecutive Cases. With Remarks on the Exanthematous Epidemic of the Spring of 1864.* By ERASMUS WILSON, F.R.S. 8vo, pp. 80. Churchill. 1864.

The Student's Book of Cutaneous Medicine and Diseases of the Skin. By ERASMUS WILSON, F.R.S. Small 8vo, pp. 51. Churchill. 1864.

The first of these works has an important bearing upon the second. The inquiry was directed to a determination of the relative frequency, the cause, and the duration of cutaneous diseases, and it has led the author to a classification of skin diseases determined by the most salient and striking characters of each disease, whether those characters be in their nature pathological, etiological, or physiological. This classification constitutes the novelty of the *Students' Book of Cutaneous Diseases*.

Of this book the author tells us that his "aim has been to simplify—to endeavour to restore to General Medicine—a department of much interest and importance; and, by furnishing the student with a clear view of these diseases, to remove them from the narrow sphere of specialism to the wider and nobler field of Catholic Medicine." Our veteran English dermatologist has been somewhat late in perceiving the true place of cutaneous pathology, and the needs of the student, and his book is rudely jostled by other manuals, written for the same purpose by much younger dermatologists. Mr. Wilson's book is not, however, on this account the less

welcome, and we do not doubt that it will hold a primary place in the regards of both students and teachers.

Of the classification adopted by Mr. Wilson a brief outline is requisite. He endeavours to show that the aims of classification are two-fold, namely: "In the first place to lay down a plan by which a knowledge of diagnosis may be most easily acquired; and, secondly, to arrange a number of diseases according to a method that will facilitate the comprehension of their nature and phenomena, and conduce to their treatment with successful results. The first of these objects," he continues, "is amply fulfilled by the classification of Willan, which is essentially a classification of diagnosis, and consequently, *par excellence*, an educational classification; while the second has been attempted by a number of authors, with varied success, under the name of practical or natural classification; the most recent of these essays being the *Chemical Classification*, which we have just endeavoured to explain."

Upon the classical arrangement all diseases of the skin at present known may be classified into *twenty-two* groups; the first in order being determined by greater frequency of occurrence. The groups are thus arranged:—

- | | |
|---|--|
| 1. Eczematous affections. | 12. Carcinomatous affections. |
| 2. Erythematous affections. | 13. Zymotic affections. |
| 3. Bullous affections. | 14. Syphilitic affections. |
| 4. Furuncular affections. | 15. Leprous affections. |
| 5. Nervous affections. | 16. Pigmentary affections. |
| 6. Vascular affections. | 17. Phytodermic affections. |
| 7. Nævic affections. | 18. Ungual affections. |
| 8. Developmental and Nutritive affections. | 19. Diseases of the hair system. |
| 9. Hypertrophic affections and atrophic affections. | 20. Diseases of the sebiparous system. |
| 10. Alphous affections. | 21. Diseases of the sudoriparous system. |
| 11. Strumous affections. | 22. Traumatic affections. |

Certain of these groups are composed of *Diseases of the Structure of the Derma*; for example—

1. Eczematous affections.
2. Erythematous affections.
3. Bullous affections.
4. Furuncular affections.

A group follows of *Diseases of the Special Structure of the Derma*, taking in the nerves, the vessels, and the contents of the vessels; thus—

5. Nervous affections.
6. Vascular affections.
7. Nævic affections.

A *third* group comprehends the morbid changes involved, and the development, nutrition, and growth, as follows:—

8. Developmental and nutritive affections.
9. Hypertrophic and atrophic affections.

A *fourth* group is founded on the presence of an existing disposition or tendency to the particular disease, that is *Diathesis*; the *diathetic diseases* being—

10. Alphous affections.
11. Strumous affections.
12. Carcinomatous affections.

A *fifth* group is founded on the dependence of the disease upon a *blood-poison*—

13. Zymotic affections.
14. Syphilitic affections.
15. Leprous affections.

A *sixth* group is composed of *Diseases of the Epidermis*—

16. Pigmentary affections.
17. Phytodermic affections.
18. Ungual affections.

A *seventh* group includes *diseases of the follicles* of the skin and their dependencies—

19. Diseases of the hair system.
20. Diseases of the sebiparous system.
21. Diseases of the sudoriparous system.

An *eighth* group includes *diseases induced by injury*—

22. Traumatic affections.

And although the individual groups are numerous, they admit of being collected under eight heads, and upon a physiological basis, a still further reduction of groups may be made, and all might be added under the four categories—

1. Dermal affections.
2. Epidermal affections.
3. Follicular affections.
4. Traumatic affections.

The dermal affections, including—

- a. Diseases of General Structure.
- b. Diseases of Special Structure.
- c. Diseases of Function.
- d. Diseases of Diathesis.
- e. Diseases of Blood-poisoning.

"It may be objected," writes Mr. Wilson, "that there is a want of unity in the clinical classification, but as unity of arrangement of cutaneous diseases is neither possible nor practical, the sooner the objection be waived the better; and the classification is none the worse in our opinion, because *four* of the eight groups are founded on a physiological, *three* on an etiological, and *one* on a pathological basis. In the class-room, or by the bed-side, we believe that the clinical classification will not be found wanting in its adaptability to the wants of the student and of the practitioner."

XV.—*Skin Diseases; their Description, Pathology, Diagnosis and Treatment; with a copious Formulary.* By TILBURY Fox, M.D. Lond. Robert Hardwicke. 1860.

If it be true that demand will originate supply, there must have been a great want felt in the special department of Dermatology, to account for the recent publication of a goodly array of text-books on Skin Diseases. The one now before us recommends itself by the extent of research which it displays, and which reflects credit on the author's industry. It collects a large amount of valuable information scattered through numerous publications, chiefly periodicals, which must have cost great labour to enable the author to present it in such condensed, but readable form. Although he carefully guards against obtruding his own views, and chiefly states those of accepted authorities on cutaneous affections, English, French, and German, Dr. Fox states his own views on any disputed points. The following quotation, in which the position of eczema is briefly summed up, will give an idea of the manner in which Dr. Fox deals with the subject of skin diseases:—

“The real conclusions seem to be this, that eczema is distinguished from its supposed allies essentially by its being a secretory disease; that the secretion is peculiar in its character, best described as stiffening linen and drying into light yellow crusts; that the outpouring of this secretion, in the first instance, is connected with the formation of vesicles; but that the latter may be rapidly produced, or imperfectly developed, or may quickly burst after their appearance, and hence also often overlooked. But we cannot refuse to admit that the tendency in all cases of eczema is the formation of vesicles, and the production of a *peculiar* secretion.”—p. 99.

XVI.—*Handbuch der allgemeinen und speciellen Chirurgie, mit Einschluss der topographischen Anatomie, Operations, und Verbandslehre.* Redigirt von Dr. von PITHA, Professor der Chirurgie in Wien, und Dr. BILLROTH, Professor der Chirurgie in Zürich. Erster Band. Erste Lieferung. 8vo, pp. 352. Erlangen: Ferdinand Enke. 1865.

Manual of General and Special Surgery, including Regional Anatomy, Operations, and Dressing. Edited by Dr. v. PITHA, Professor of Surgery at Vienna, and Dr. BILLROTH, Professor of Surgery at Zurich. First Volume. First Part. Large 8vo, pp. 352. Erlangen. 1865.

The undertaking, of which this is a first instalment, promises to be a “System of Surgery” of the most complete kind, and to embody a full account of the teachings and practice, not only of all the great schools of Germany, but of those of other countries also.

Among the various contributors of note, the names of Professor Esmarch, Messbaben, Simon, Von Tröltsch, and O. Weber, appear upon the cover, and are abundant guarantees for the excellence of the work with which they are associated. The part at present issued comprises a brief introduction upon the history of surgery, by Professor Haeser of Breslau, occupying only twenty-six pages, and the commencement of a dissertation by Professor O. Weber, upon the diseases of the tissues and their influence upon the organism. This dissertation is to be divided into the two heads of local and general derangements; and the first part contains an account of some of the former, including hyperæmia, inflammation, and the restoration, hypertrophy, and degeneration of tissues. These subjects are treated with all the abundance of knowledge and the copiousness of detail for which Germany is justly famed, and the author has evidently spared no pains or research in obtaining materials from every source. The several sections are preceded by copious bibliographical references, and authors and observers of all countries are freely quoted or referred to in the text.

Among the advantages of a knowledge of the German language to medical practitioners, not the smallest is the cheapness of German scientific literature. In Holmes's excellent *System of Surgery* there are no illustrations, because the cost of them would have rendered the price of the book almost prohibitory to the generality of intending purchasers. The "*Manual of Surgery*" will consist of four volumes of very large paper, containing about 700 pages each, and with numerous woodcuts, which are not finished in the best style of art, but are still great helps to the ready comprehension of verbal descriptions. These four volumes will cost forty-eight shillings. Uniform with them will be published an *Atlas*, containing one hundred and thirty-six steel engravings and fifty-two plates of lithographic outlines, with brief descriptive letter-press, at twelve shillings more.

We shall reserve until the completion of the work, or, at least, until the appearance of some important special article, any detailed review of it, and we at present only desire to call attention to the fact that its publication has commenced.

XVII.—*Military Surgery*. By GEORGE WILLIAMSON, M.D., Surgeon-Major 64th Regiment. 8vo, pp. 255. London, 1863.

Dr. Williamson has done good service to military surgery by reprinting this work in its present form, and with a new title. In its original form the work was known as *Notes on the Wounded from the Mutiny in India*. Enriched with Dr. Williamson's observations on the different forms of injury, which are illustrated by the cases he records, the work, as it now appears, will prove most valuable to the military surgeon. All soldiers of cavalry and infantry of the line, invalided on account of wounds, pass through the invalided dépôt at

Chatham. "It is there then," says Dr. Williamson, "that probably the best opportunity is to be found for ascertaining the results of different wars, in the several classes and species of wounds, and in the proportion which those classes and species bear to each other." The collection and record of such results seemed to Dr. Williamson to be very desirable, and he availed himself of the opportunities which Chatham afforded as regards the wounded by the mutiny in India. The results of his labours are contained in the present work. Although the absence of similar records with regard to previous wars has prevented a wide comparison of data, Dr. Williamson has elicited many points of great moment. We would refer more particularly to the large number of recoveries of patients with good, useful limbs, after gunshot compound fracture of the femur, as compared with the number of thigh-stump cases, and the total by all wounds.

"It is taught," our author says, "by most military surgeons that, as a rule, immediate amputation should be performed in all confirmed comminuted fractures of the femur; and that, by attempting to save limbs, more patients have lost their lives. It is also recommended in all cases of gunshot fractures of the middle and lower thirds of the femur, but especially in the middle third, that amputation should be performed; but in the same injury in the upper third the limb should be preserved, as it has been found that amputation in the upper third of the thigh is almost certainly fatal, so that such a severe operation is not considered advisable when the prospect of success is so small, and by retaining the limb the patient is much more comfortable, with equal chances of saving his life. It may be mentioned that one case of amputation of the upper third of the thigh arrived from India; it was so high up that the stump could not be retained in the bucket of the artificial limb.

"It appears from the result of the Schleswig-Holstein war, that a considerable number of these cases were preserved with very useful limbs. The number which have arrived from India is very large in proportion, viz., 13 cases of compound comminuted fracture of the femur to the total wounded, 842. Of 2296 discharged the service at Chatham, in consequence of wounds received in the Crimean war, there were only eight recoveries with the limb on. The success of these cases from India may, in part, be attributed by some surgeons to the ball of the old musket being smaller, and not producing such a severe fracture as that used by the Russians during the late war; but more to the *dooley* as a *means* of conveyance; and in part to the army having been better supplied with good food, clothing, &c.

"Generally, cases of gunshot wounds progress more favourably in hot than in very cold climates, especially when great attention is paid to cleanliness. These fractures are, however, in my opinion, as severe as could be caused by any Russian ball, viz., in M'Carter's case, produced by round shot, and Carty's, where there was a double fracture. And, looking at these cases, it appears to be still an open question as to the necessity of immediate amputation in all cases of gunshot fractures of the femur. When the fracture is close to the knee, or if the bone is split into the joint, amputation will be necessary; when the bone is simply fractured, and not splintered to any great extent, the ball having traversed the limb, it seems to be advisable, under favourable circumstances as to after-treatment, to try and save the limb at whatever part of the bone the fracture has taken place; when the femur is more extensively shattered recourse must, of necessity, be had to amputation. A great deal must also depend upon the kind of projectile: when from round shot, there is generally great comminution, and little hope

can be entertained of saving the limb ; but recoveries under such unfavourable circumstances do occasionally take place, as illustrated by the case of M'Carter, 64th regiment. The fact that three men were sent to modified duty shows that they must have retained very useful limbs, so as to be able to carry messages, and to act as orderlies ; more than three might have been sent, as far as the condition of the leg was concerned.

"The advantage of the *dooley* over the best-constructed *ambulance*, for the conveyance of sick and wounded over rough roads during active military operations, is well shown in the result of these cases of compound comminuted fracture of the femur. All regiments in India have a certain proportion of *dooleys* and *dooley-bearers* attached to them permanently in the time of peace, and when on the line of march and on active service they are of course increased. *Why should not Government enlist and organize in India a corps of trained dooley-bearers for service with our regiments in European wars ?*

"This appears to me to be a measure which could be carried out without much difficulty, if sufficient inducements as to pay, good treatment when in the service, and the promise of a pension when discharged, and sent back to their homes in India, were held out to them."

Dr. Williamson has taken advantage of the valuable preparations in the Museum of the Army Medical Department to illustrate his work, detailing every specimen of gunshot wound up to 1859. These preparations were arranged and catalogued by him in 1845. Not the least merit of Dr. Williamson's book is the beautiful series of lithographic plates which ornament it.

XVIII.—*Surgical Experiences: the Substance of Clinical Lectures.* By SAMUEL SOLLY, F.R.S., Senior Surgeon to St. Thomas's Hospital, Member of the Council, and late Professor of Anatomy and Surgery in the Royal College of Surgeons of England, &c. 8vo, pp. 656. Hardwicke. 1865.

"No obligation," says Mr. Solly, "is more plainly laid on a hospital surgeon, by virtue of his office, than that of publicity." He looks upon the hospital surgeon as a steward who is bound from time to time to render an account of his stewardship to the profession. He does not hold office for himself alone, but in trust for the public and his brethren ; and it is his duty to make known for the common good such lessons of success or of failure as he may have gathered from the great field of observation open to him.

Accordingly, influenced by this lofty estimate of the hospital surgeon's professional responsibility, Mr. Solly makes public, in a handsome volume, the results of many years' teachings at the bedside. With a courtesy, as charming as (it must be confessed) it is rare, he dedicates this work to his dressers, past and present—a goodly list of whose names is added to the preface.

Mr. Solly's will hold a foremost place among the too-limited series of English works on clinical surgery. It would seem, however, as if its author thought a *quasi* apology necessary for publishing it in its pre-

sent fashion. "The form," he says, "in which the present work is cast appears to me to possess greater authenticity than could be attained by other and more pretentious methods. In recording surgical experiences, it has been my object rather to follow, simply and honestly, the teaching of facts as they came under my own notice, than to aim at the theoretical completeness of a systematic treatise."

We are especially grateful to Mr. Solly for having adopted this course, and we would fain hope that his excellent example will not be altogether lost upon other hospital surgeons or physicians who may be proposing to handle the pen. If there be one evil more conspicuous than another in English medical literature, it is the concoction of complete treatises. This evil would almost seem to be increasing, if we may judge from a recent irruption of manuals and handbooks, and treatises, as inutile as mediocre. Why is the student and junior practitioner to be fed upon a trashy diet of manuals and pretentious treatises, while sound clinical works—the very marrow of teaching—are of the rarest? Is it because hospital surgeons and physicians as a rule are less heedful of that duty of "publicity" which Mr. Solly insists upon? We fear that the answer to these questions must be in a great measure sought in this direction. Our hospital medical-staffs are the great repositories of clinical experience; but where is the record of their clinical teachings?

The subjects treated by Mr. Solly are numerous. Among them are Diseases of the Spine, Injuries of the Head and Spinal Cord, Concussion of the Brain, Paralysis, and in especial, Scrivener's Palsy, Diseases of the Joints, Partial Amputation of the Hand, Excision of the Maxilla Superior, Hernia, Ruptured Bladder, Stricture, &c.

Among this wealth of matter, it is not an easy task to determine in what manner best to show the fashion of Mr. Solly's book. We elect, however, that he should speak for himself, and dip almost at random into his pages.

Mr. Solly is writing of *rheumatic cancer of the spine* :—

"I need hardly insist," he says, "on the importance of a correct diagnosis and prognosis in these cases; and if it is important for you to distinguish between lateral curvature and angular curvature, so it is even still more important that you should distinguish between ordinary or *strumous* caries of the spine, and the *rheumatic* caries of Sir B. Brodie; for I am convinced that the two diseases are essentially different, both as regards their origin, progress, and termination, and the treatment required for their cure; and again, between the latter, and disease of the cord independent of disease of the bone. Sir B. Brodie, in a lecture published in the *Lancet* of the 30th of December, 1843, has remarked that rheumatic caries is always accompanied with pain; scrofulous caries frequently, not so invariably; and in paraplegia from disease of the cord independent of disease of the vertebræ, there is an entire absence of pain. The history of each case will also assist you—the presence or absence of a rheumatic or of a scrofulous diathesis—the moral history of your patient, independent of disease of the cord being generally the result of excess in venery by persons leading sedentary lives and engaged in continual mental exertion. But in these last cases—and I have seen several—there is no pain on tapping the spine.

But, on the other hand, there are cases of spinal disease in which the diagnosis is not so easy. The patient suffers severe pain on pressure over the spinous processes, with total loss of power of the legs, but without any irregularity of the vertebræ, or other evidence of the existence of disease of the bones. The most anomalous symptoms, however, generally occur in females; and when we find in our examination of these cases, that there is no evidence of a rheumatic tendency previously, that they never have suffered from rheumatic pains, and that the tenderness on pressure is not confined definitely to any particular portion of the spine, severe pressure not inflicting more pain than slight; again, the pain shifting sometimes in one portion of the spine, sometimes in another; and if, in addition to this, you find clear signs of a hysterical diathesis, you may generally refer that ailment to hysterical origin. This term, however, is liable to mislead. Hysterical complaints are often set down as altogether imaginary, but such is not the case: they are often most serious to the patient, and most harassing and difficult to the practitioner"—p. 48.

Mr. Solly believes that rheumatic caries "is a local mollities ossium, *rubra et fragilis*."

The following observations refer to paraplegia consequent upon permanent spinal exhaustion:—

"The disease commences with slight numbness of the lower extremities; this is followed by some loss of power; there is no pain in the spinal region at all; when you examine, then you may rap the spine from the neck to the rump, and the patient does not shrink. You may apply the hot sponge, but this elicits no evidence of disease of the vertebral column.

"The history will assist you if you strike the right key. You find no evidence of your patient having ever received any injury to the spine. He cannot account for it at all. If, however, you ask him whether he has had much sexual intercourse, he must say, if he is honest, yes; but most probably he will not acknowledge it immediately. When you tax him directly with not having been satisfied with the caresses and charms of one gem, that two claimed him for their own, and that his animal pride would not permit him to stint them, he will generally acknowledge the soft impeachment. If, on the other hand, he says indignantly that he never had connexion with a woman in his life, it is almost certain that he is a victim of that dreadful delusion—masturbation.

"In the treatment of these cases you must avoid all antiphlogistic measures, for they only do harm. The first thing is to stop the exciting cause, and this is often the most difficult part of your task. I have known men of sound sense in all other matters, men whose judgment is of the greatest value to their clients, such slaves to the venereal appetite and their own ideas of pleasure, that they would submit to any plan of treatment that you like to propose, yet would not abstain from copulation, or give up their ordinary exercise and mental employment. I remember once saying to a patient, who consulted me for this malady, and whom I found perfectly deaf to all advice on this point, 'The best thing that could happen to you would be to be pitched out of your phaeton, and to have a bad compound fracture of the leg, which would confine you to your bed and your back for at least two months.' Now, it did so happen that this gentleman met with an accident, though, unfortunately for him, not so serious as to confine him for more than a month or six weeks; but even this rest did him so much good, and he rose so much better, that he forgot all his good resolutions, pursued the same course again, and is now perfectly, and, I fear irrecoverably, paraplegic.

"Unless you enjoin in these cases rest—bodily, mentally, and erotically—you can do little or nothing with them; and if your patient will not submit to it, you had much better take your leave without prescribing, for all the medicine in the Pharmacopœia will do no good without entire rest.

"As regards medicine, I have found ten to fifteen drops of the tinct. lytœ., with from two to four grains of sulphate of zinc, the best. A generous, but not stimulating diet, must be advised."—p. 63.

In his observations on injuries of the head and spinal cord, Mr. Solly has the following remarks concerning blood-letting:—

"On reading over the remarks which I made on blood-letting in the last section, I feel bound now to put on record my opinion after more than thirty years' experience on this subject. I fear that, as a profession, we are too much losing sight of blood-letting as a remedial agent. I am sure that nature often warns us of our neglect by instituting severe hæmorrhage; and that many a valuable life has been saved by nature's conservative hand.—p. 137.

Mr. Solly relates several cases illustrative of the good effects of blood-letting, and he expresses his conviction that "local blood-letting is infinitely more effective than venesection from the arm."—p. 641.

Apropos of amaurosis produced by choroiditis, Mr. Solly says:—

"The great treatment of success in the treatment of these chronic cases, as indeed it is in all chronic cases, is not to attempt too much. Be contented with a very slow process, but, being firm in your view of the pathology of the case, persevere. Do not be turned to the right or to the left, either by the stupid and officious sister, presence of the kind friends of your patient, or by his own impatience. You may often be obliged to give mercury in minute doses for one or two months before there will be any improvement in vision, but this must not discourage you; go on and you will have the satisfaction of seeing your patient ultimately recover. But you must stop short of salivation."—p. 154.

There are some instructive remarks on the use of the small catgut bougie:—

"This is the great secret in the use of the thread-sized catgut bougie: it must be handled most delicately, turned in the urethra, something like a corkscrew, till the hole through which the urine escapes from the bladder is pitched upon; and this is immediately felt by the instrument passing forwards without any sensation of obstruction. The catgut bougie must never be used with the idea of breaking down a stricture or pushing through it, but, if I may so express it, coaxing it into the bladder. Whenever you find the bougie spring back, you must stop and give it a little twist between your forefinger and thumb: you may often have to work for half-an-hour or more in this way, without being able to hit off the opening; but patience and delicate handling will do a great deal. I assure you it is worth taking some trouble to relieve a fellow creature effectually of such a serious malady without the use of the knife. Having passed the stricture, and entered the bladder, I desired that the bougie should be retained there for an hour. I attach great importance to the retention of the bougie; and with the catgut bougie there is this additional advantage, that the bougie swells to twice its original size. I attach so much importance to the retention of the bougie, that in private practice I have had an additional one made on purpose for such patients."—p. 509.

And so we might quote page after page of instructive and suggestive matter, but the above will suffice in some measure to show the value of Mr. Solly's *Surgical Experiences* to the active practitioner.

XIX.—*Lectures on the Pathology and Treatment of Lateral and other Forms of Curvature of the Spine.* By WILLIAM ADAMS, F.R.C.S., Surgeon to the Royal Orthopædic and Great Northern Hospitals, President of the Harveian Society. 8vo, pp. 334. Churchill. 1865.

These lectures were delivered at the Grosvenor-place School of Medicine during the Session 1860-61. Their main object is to establish the treatment of spinal curvature upon a more exact knowledge of its pathology; and to this end especial attention is devoted to an examination of the connexion between the external appearances presented during life, and the symptoms which accompany the severer forms of this deformity. Mr. Adams brings to his task a very comprehensive acquaintance with pathological anatomy, and an extensive chemical experience of the malady to which he would apply that knowledge. The result of his observation has led him to differ in many important points from the standard writers on the subject. His interpretation of some of the cardinal questions of spinal curvature is diametrically opposed to that which is commonly received. Resting entirely upon clinical and pathological data, this interpretation commands earnest attention; and if the arguments do not necessarily lead to a conviction of their soundness, they at least claim admiration for their suggestiveness.

For example, it is a canon of orthopædic surgery, founded on the theory that the spinal muscles take an active part in maintaining the erect position, that unantagonized muscular action, from whatever cause arising, is a fertile cause of spinal curvature. This doctrine, and the physiological theory at the bottom of it, Mr. Adams refuses to admit. Clinical, pathological, and physiological observation he asserts are opposed to both the one and the other. The capability of the erect position, he argues, rests in the construction and natural curves of the spine itself, its flexibility being very limited. He concurs with Mr. Bishop in the opinion that the erect position of the body is that in which there is the state of least muscular action. "The general principle of economizing muscular force in the erect position of the body, is," he says, "to be traced not only in the spinal column, where it is most conspicuous, and the muscular force required is reduced to its minimum, but also in the mechanism of the movable articulations of the extremities, as in the knee and hip-joints; in the spinal column it is clearly evidenced by the large size and form of the articular surfaces of the bodies of the vertebræ; but in the knee and hip-joints a much larger expenditure of muscular force must be required in maintaining the erect position of the body; and, indeed, in these articulations, the maximum of muscular force in comparison with the other articulations of the body is required. To compare these articulations, then, physiologically and

pathologically, in explaining the causes and mode of production of lateral curvature of the spine, as most writers on spinal curvature have done, appears to me to be an error requiring but little reflection to correct. . . . (p. 52.)

"The muscles of the spine," he proceeds, "are in a state of the least action when the spinal column is in the erect position. Instead of the muscles of the spine in the erect position being in a state of *active tension*, as assumed by some writers, it would be more correct to describe them as in a state of *vigilant repose*, ready on the instant to check and limit the flexion of the spine when its balance is disturbed, and thus assist in preserving the general equilibrium of the body in any of the various attitudes, such as the dancer or the gymnast may assume." (p. 58.)

It is only when the equilibrium of the spinal column is disturbed that the muscles are called into action, those in the convexity of the curve being placed violently on the stretch, those in the concavity remaining soft and flaccid. "The latter circumstance," says Mr. Adams, "is practically well worthy of remark, and appears to me to prove that the *spine is not drawn* into a curved position by the active contraction of muscles in the same way that the arm or leg is flexed, when the biceps and hamstring muscles respectively become prominent and tense, but that the spine bends passively as a flexible column, in consequence of disturbance of its equilibrium, the result either of some alteration in the angle of its support—some tilting of the pelvis—or of some alteration in the distribution of the weight it has to support above; a bending of the head or the uplifting of one arm, &c." (p. 55.)

The important bearing of this doctrine upon certain systems of treating spinal curvature is obvious. Thus, receiving it, the gymnastic system must be rejected as capable of curing lateral curvature of the spine. Not that Mr. Adams sets aside muscular exercises in the treatment of these cases. When used judiciously, with other means, in slight cases, associated with muscular debility, he has no doubt of the great value of systematic gymnastics. But he remarks that "certainly less than half of all the cases of lateral curvature which have passed under his observation have neither been preceded nor accompanied by muscular debility, and that even where muscular debility has existed, other causes, both constitutional and local, have also existed; and, according to the views of the mode of production and general pathology of lateral curvature which I have adopted, appear to me to be of not less importance than the condition of muscular debility." (p. 289.)

One of the most important portions of Mr. Adams' work is that devoted to a demonstration of the occasional existence of internal without any external curvature of the spine, or lateral deviation of the apices of the spinous processes, either in severe or slight cases, and the means of detecting this deviation. Rotation of the vertebrae in spinal curvature has been described by previous writers; but the view adopted by Mr. Adams is that "rotation takes place at the commencement of the spinal curvature, and forms an essential part of the series of structural changes which must necessarily exist in slight as well as severe cases, and that the external lateral deviation occurs

subsequent, rather than previous, to the rotative movement." (p. 116.) The practical importance of this doctrine, and the light which it throws upon certain obscure cases of disease, must be learned by a reference to Mr. Adams' own pages.

This book, indeed, is one of great interest, theoretically and practically. It disturbs in a very welcome fashion the somewhat stagnant condition of orthopædic surgery, and raises several important points which require careful reconsideration. A work of this character was much needed, and the one before us is eminently calculated to excite a wider professional interest in distortions of the spine.

XX.—*Curvatures of the Spine: their Causes, Symptoms, Pathology, and Treatment.* By BERNARD E. BRODHURST, F.R.C.S., Assistant-Surgeon St. George's Hospital and to the Royal Orthopædic Hospital. Second Edition. 8vo, pp. 93. Churchill. 1864.

This "second" edition of Mr. Brodhurst, according to the author, possesses nothing of the "first" edition except the title. It has been entirely re-written, and although still of peculiarly restricted dimensions for so complicated a subject, is "much enlarged." The woodcuts are also new. So far as the publisher is concerned, the book is unexceptional; the paper is, in its goodness and rarity, such as gladdens the eye of a medical reader in professional books; the typography is admirable, and the illustrations, executed by Bagg, could not be excelled. But the text of the book is of the briefest, and it may be questioned whether in his attempt to familiarize the ordinary professional mind with spinal curvature, the author has not concentrated the mental aliment to the extent of indigestibility. Taking the book as it is, however, it is an excellent summary of the most received opinions on and approved methods of treatment of spinal curvature, ballasted by the opinions of the author.

Mr. Brodhurst does not deal so cavalierly with the theory of muscular action as a cause of spinal curvature as Mr. Adams. He speaks "of loss of muscular power" in conjunction with an unsupported pendulous abdomen in one case causing anterior curvature. "Partial paralysis" is assigned as a cause of posterior curvature." Of lateral curvature he says:—"Paralysis of one side of the trunk always gives rise to lateral curvature of the spine, the healthy muscles drawing the spinal column away from the mesial line. The length of the curve will depend also on the extent and amount of the paralysis: it may be a single curve, all the muscles of one side being paralysed; or it may be compensated by a second curve. After amputation of the arm, for instance, the muscles of the shoulder will become atrophic, and, in consequence, the opponent muscles exercising undue power on the spinal column, will curve it in the direction of their combined forces." (p. 41.)

Mr. Brodhurst refers to the common occurrence of lateral curvature in certain trades in which one arm is chiefly used; and, further,

he says, "*spasmodic action* of the muscles of the back, neck, and shoulder occasionally gives rise to scoliosis." (p. 46.)

Leaving this debated point, we quote the following observations on the mechanical treatment of lateral curvature:—"When curvature has become confirmed, mechanical means are absolutely essential to remove the curve. And this having been determined on, it will scarcely be necessary further to insist that of a series of curves, the primary must first be acted on. Other curves which may be formed are merely compensatory of the first; so that, to treat one of these in lieu of the primary curve, is much the same as with disease to treat a symptom only.

"It requires only a slight knowledge of mechanics to render it quite certain that in acting on a curve to remove it, pressure should not be made on the greatest convexity of the curve. Pressure in this position is quite unequal to remove a curve; it will flatten the ribs, but never lessen the spinal curve. . . .

"It is evident that such pressure as is necessary to be made should be so applied that it may produce the greatest effect in straightening the spine, with the least injury to those parts which are acted on. This may be done by acting on the lower portion of the convexity of the primary curve obliquely upwards, and on the upper portion of the secondary curve obliquely downwards. This rule must be observed when the curves are formed from above downwards. But when they are formed from below upwards—for instance, lumbar and dorsal, or lower dorsal and upper dorsal, &c.,—it is necessary to act on the lower portion of each curve.

"The pressure should, as much as possible, be made to follow the curve of deviation of the parts themselves; and, consequently, it should never be directly lateral. By using extension in the manner indicated, curves, which cannot otherwise be treated, may be unfolded. This unfolding process is slow; and this will not excite surprise, when it is remembered what are the pathological changes which have taken place. Rapid effects are impossible, and should not be expected." (p. 86.)

XXI.—*Etudes Cliniques sur l'évacuation répétée de l'humour aqueuse dans les Maladies de l'Œil*. Par CASIMIR SPERINO, Professeur d'Ophthalmologie à l'Université de Turin. Rédigées avec le concours du Docteur CHARLES REYMOND. 8vo, pp. 496. Turin, 1862.

Clinical Observations upon Repeated Evacuations of the Aqueous Humour in Diseases of the Eye. By CASIMIR SPERINO, Professor of Ophthalmology in the University of Turin. Collected with the assistance of Dr. CHARLES REYMOND.

It has long been known to ophthalmologists that Dr. Sperino was extensively practising evacuation of the aqueous humour in many diseases of the eye; and the volume before us contains a very full

record of his experience. His views may be briefly stated by saying that the repeated evacuation of the humour maintains the eye in a state of diminished tension, and affords opportunity for the redress of any derangements in the natural balance of its circulation. It also causes a vast increase in the rapidity with which the aqueous humour is secreted; and thus it effects a kind of local depletion on the one hand, and on the other removes, at a single stroke, any morbid products that the aqueous humour may have dissolved, and that would otherwise be slowly carried away by the bloodvessels. All the chronic internal ophthalmiæ, all the varieties of glaucoma, all the diseases of the cornea associated with increased tension, and many forms of cataract (those, namely, in which the histological structures of the lens have not undergone degeneration), are the chief classes of cases likely to derive benefit from such an action; and it is in these, accordingly, that Dr. Sperino has chiefly employed his method and effected his cures.

In operating he uses a common cutting needle, slightly curved on the flat, and about three millimetres in width. It is introduced with its concave side in front, to avoid all risk of wounding the iris, and is at once withdrawn. The evacuation of the humour is then effected by light pressure, with a metal or whalebone probe, on the posterior lip of the little puncture, so as to make it gape; and this pressure is repeated, or the puncture, if necessary, reopened by introducing the probe, whenever it is wished to evacuate the humour afresh. The exact point of puncture is not important, except that, if it be forward on the cornea, it will leave a cicatrix, and, if it be too far backward, it is apt to be obscured by swelling of the conjunctiva. Dr. Sperino therefore selects the corneo-scleral junction. At this point, especially if the conjunctiva or the pericorneal vascular circle be hyperæmic, a little blood will escape at the time of puncture, and at each of the first two or three evacuations; but this is an occurrence rather desirable than otherwise. Dr. Sperino punctures at the temporal extremity of the horizontal corneal meridian, in order that the opening, which is usually invisible or nearly so, may always be readily found, even if some of the subsequent evacuations are made by a second operator.

After each evacuation it is desirable to cover the eye with cold compresses for a few hours, to check any tendency to inflammation; and, with this exception, the evacuations need not interfere with the ordinary pursuits of the patient. They are repeated frequently, sometimes daily, and in some cases they have been very numerous—reaching even from fifty to a hundred. They appear never to have been hurtful and very generally indeed they may have been curative, in all the classes of cases mentioned above.

The applicability of this method of treatment seems to depend, in great measure, upon its freedom from risk. In Dr. Sperino's hands it is said to possess this merit. Mr. Hildige, we believe, has tried it in a few cases, and was compelled to abandon it in consequence of the inflammation it excited. Possibly he omitted some precaution; possibly his want of success was due to climatic influences. In this country we have not heard of the method being sufficiently tested for any certain conclusion to be formed about it.

On account, however, of its tediousness and costliness, it seems hardly likely, we think, to supersede iridectomy or cataract extraction. In some cases of subacute irido-choroiditis, and in obstinate corneal ulcer in young people, we should think it eminently deserving of a fair trial at the hands of English surgeons. In cases of cataract, moreover, in timid patients, it has the advantage that its value may be tested without much loss of time. Although absolute cure is tedious, Dr. Sperino says that decided improvement is manifested very early, in cases where it is manifested at all.

XXII.—*The Optical Defects of the Eye, and their consequences, Asthenopia and Strabismus.* By JOHN ZACHARIAH LAURENCE, F.R.C.S., M.B. (Lond.), Surgeon to the Ophthalmic Hospital, Southwark, &c., &c. 8vo, pp. 112. London: Hardwicke. 1865.

The recent researches of Professor Donders and his fellow-labourers have been brought before English practitioners, not only by the elaborate and complete work published by the Sydenham Society, but also by treatises of smaller size and less pretension, intended to display chiefly the practical bearings of the subject, and to meet the requirements of men who desire to learn what to do for their patients with as little loss of time as possible. Among such treatises the one before us is, we think, the best; because it is not a mere abbreviated translation of German writings, but the work of a man who has evidently thought out and mastered the subject for himself, and familiarized himself with all its bearings by daily practice. This is shown in many ways, and chiefly by the constant recognition of small matters that are sources of embarrassment or error in early attempts to test the acuteness, or character of vision. Moreover, Mr. Laurence begins at the beginning, and deals with the very elements of his subject in plain and simple language, and often with great felicity of illustration. The action of lenses, and the way in which they assist vision, are rendered perfectly clear; and this part of the book is copiously illustrated by appropriate diagrams. Mr. Laurence may fairly claim to have cut out and simplified just so much of the science of optics as was indispensable to his readers; so that any who do not come to his pages ready fortified with this knowledge will find their wants in respect of it provided for. Within the small compass of his book it was impossible to do more than give a general outline of so large a subject; but Mr. Laurence has shown great skill in selecting both the matters treated of and the matters omitted. Anyone desirous of qualifying himself to prescribe spectacles for ordinary cases, will find all the necessary information in a most compact and accessible form; and those who seek for more than this will have recourse to the great work of Professor Donders. Mr. Laurence has neglected nothing that could be of real use to his readers, and gives an account, for instance, of Javal's instrument for the binocular testing of astigmatism, although a description of it was not published until his sheets were actually passing through the press.

Mr. Laurence writes so clearly and plainly, and selects his subject matter with so much judgment, that we are sorry to accuse him of not writing elegantly. We only do so, indeed, in the hope that he may long continue to contribute to medical literature, and that he may think gracefulness of style a matter worthy of more attention than it has hitherto received at his hands. Many of his sentences would be much improved by simple transposition of their words, which are arranged in very un-English order, the most common error being to place an adverb between the preposition of, the infinitive, and the verb itself: "I wish to here distinctly state," is Mr. Laurence's version of "Here I wish to state distinctly." It is difficult to repress a smile on finding that the descriptive sentence, "An eminent member of our profession," refers to—*Galen*! These small blemishes, however, in no way detract from the sterling value of the work, which we very cordially commend to the perusal of our readers.

XXIII.—*The Ophthalmoscope: its Varieties and its Use.*

Translated from the German of Dr. Adolf Zander. By ROBERT BRUDENELL CARTER, M.R.C.S.E., Fellow of the Royal Medical and Chirurgical Society. With notes and additions by the Translator. London: Hardwicke. 1864. 8vo, pp. 225.

Mr. Carter has done sterling service to British physic by the publication of this work. Compendious in form, masterly in execution, and exhaustive in detail, Zander's treatise is unquestionably the best German, perhaps the best Continental, text-book on the ophthalmoscope. Mr. Carter's translation bids fair to hold the same high position in English medical literature which the original work holds on the Continent. The author has executed his task so well as to give to the translation a greater value than the original. He has rendered Zander's text, with loving care to retain the spirit of the original, and he has spared no pains in making such additions as were made necessary by the advance of ophthalmoscopic science since the publication of the last German edition. In making the additions, Mr. Carter has availed himself of the counsels of Dr. Liebreich and Professor Wecker, and thus he is able to say confidently, that nothing important has been omitted.

The work is divided into six sections. Section the first, treats of the varieties of the ophthalmoscope, and the principles on which they depend. Section the second, is devoted to the examination with the ophthalmoscope. Section the third, deals with the ophthalmoscopic appearances of healthy eyes; and section the fourth, with the ophthalmoscopic appearances of diseased eyes. Section the fifth, briefly touches on the relations of the ophthalmoscope to forensic medicine; and section the sixth, sets forth the influence of the ophthalmoscope upon the treatment of ocular diseases. The sixth section is not comprised in the original, but has been added by Mr. Carter, "in deference to the practical character of the English mind."

Of this section, and of the other additions made to the original by Mr. Carter, it may be said that they show a thorough mastery of the subject to which they relate.

We quote Mr. Carter's remarks on the choice of an ophthalmoscope :—

"The beginner must consider it—1, as between English and foreign instruments ; 2, as between the binocular and the monocular ; 3, as between the varieties of either class.

"The observers who require a fixed instrument must always be a minority, and will be always limited to surgeons in consulting ophthalmic practice, to those who have to instruct students, and to those who have opportunities and leisure to make careful drawings of the appearances they observe. For all these purposes, the large ophthalmoscope of Liebreich answers admirably ; but I should think the modification of it contrived by Follin (*see* p. 33), would be in many respects more convenient. The mobility of the object-lens itself, as well as of the power to draw the patient gently towards it, or to make him recede from it, without altering the field of vision, and without moving the eye of the observer, would often be highly valuable.

"Such instruments should be obtained from English makers. The foreign ones that I have seen have been at once dear and bad. The threads of their screws are always defective ; the parts that should be immovable slide downward, in tardy and reluctant obedience to the law of gravitation. By the time that all the faulty portions have been replaced in this country, the instrument will be far more costly than one originally of English construction, made to work as smoothly and easily as a microscope from the first. I trust my readers will take warning from experience.

"I have found that the most convenient method of using a stationary ophthalmoscope is to attach it to a small table constructed for the purpose. My own has a surface of about a foot square, and is supported by a strong central column, heavily weighted at the base. In practice, this arrangement possesses many advantages over the corner of a larger table.

"The smaller foreign instruments are generally cheaper than the English, often much cheaper, and, if of simple construction, are fully equal to them. The hand-ophthalmoscopes of Liebreich, Coccius, and Zehender, are largely imported from Berlin, and may be obtained of excellent quality from any instrument-maker or optician. The binocular instrument of Dr. Giraud-Teulon is made chiefly by MM. Nacet et Fils, and, in consequence of the number they manufacture, certainly cheaper than elsewhere. Messrs. Murray and Heath's instrument has been made only by themselves.

"The difference between the effects produced by binocular and monocular ophthalmoscopes is very considerable, and for a beginner or an inexperienced observer, is very important. In order to estimate this difference correctly, it must be remembered that the difficulties of ophthalmoscopy are twofold. There is, first, the difficulty of seeing ; and then there is the difficulty of interpreting what is seen. Of these, the first is the same with all instruments ; but the binoculars reduce the second to a minimum.

"In using the monocular ophthalmoscope of Coccius or Liebreich for the inverted image, in spite of abundant light and perfect definition, the details of the picture appear to be all in the same plane. The vessels of the retina can be distinguished from those of the choroid by colour and direction, but not by any appreciable difference in their position. The depressions formed by choroidal atrophy or posterior staphyloma, and the elevations from sub-retinal hæmorrhage or effusion, present colours which contrast with those of the general field, but scarcely any appearances by which, prior to reflection,

their sunken or varied position can be positively determined. Even the cupped optic disk, the most marked of the surface changes of the *purans oculi*, betrays itself chiefly by the bending of the vessels at its margin, and, by inexperienced observers, is often mistaken for an elevation. It may fairly be said, I think, that the limited power of one eye to furnish data for correctly estimating relief is the chief source of difficulty in the interpretation of ophthalmoscopic appearances.

"With a good binocular ophthalmoscope, this difficulty vanishes. The difference between the appearances presented by the two methods is like the difference between the appearance of a tree growing in a field and a tree painted in a picture. Not only is the depressed optic nerve immediately recognised as an unmistakable cup or cavity, but even small effusions of blood, or lymph, or serum, on the one hand, or patches of atrophy on the other, present aspects that are conclusive with regard to their relations to the general level of the field. The vessels of the retina, too, are seen to stand out from, and to be distinctly on a plane anterior to, those of the choroid—which, again, in young, light eyes, with good illumination, may be distinctly traced to different strata of the membrane.

"For those surgeons who have obtained a thorough mastery of the older forms of the instrument, and with whom the process of eliminating optical illusions, or of using the data by which relative position can be determined, has become almost instinctive, the binocular ophthalmoscope is only so far advantageous as the picture it presents is more beautiful. But for less skillful observers it is of high practical value.

"There are few ophthalmoscopic questions more important, or having a greater bearing upon diagnosis and prognosis, than to determine whether a given mass of pigment be infiltrated among the retinal tissues, or deposited beneath the retina in the choroid. It is of equal importance to be able to recognise with certainty the commencement of serous sub-retinal effusion. By binocular vision, all these conditions can be determined at a glance; by monocular, if at all, only after protracted and possibly hurtful examination. It follows, I think, that by observers who desire to learn the use of the ophthalmoscope quickly, and to avoid errors of interpretation after having had but small experience, the binocular instrument should be unhesitatingly preferred.

"For the erect image, the binocular ophthalmoscope possesses little advantage over the older forms; and even when adapted for lateral illumination, it is not so easy of use. For this purpose, therefore, it is not calculated to supersede the ophthalmoscope of Zehender; but it is still quite available in the hands of an observer who will take the trouble to master the difficulties of manipulation. For the erect image, it will generally be needful slightly to approximate the prisms. The respective merits of the different binoculars have been sufficiently noticed in the former section.

"Before purchasing a binocular ophthalmoscope, it is prudent to make certain of the possession of binocular vision. This is not universal, and those to whom it is wanting are often unconscious of their deficiency. The test is very simple. The eyes should be directed towards a candle-flame, or other convenient small object, at a distance of eight or ten feet; and a prism of 16° or 20°, with its angle towards the nasal side, should be held before one eye. With binocular vision, the result will be either two images of the object, or an internal strabismus very manifest to a bystander. If neither of these consequences follow, a binocular ophthalmoscope will be wholly useless.

"With regard to the choice of a monocular instrument, I find myself compelled to differ from Zander in two particulars: first, with regard to the

utility of the simple mirror ; and, secondly, 'with regard to the superiority of a concave mirror for the inverted image.

"My own earliest attempts at ophthalmoscopic investigation were made with the simple mirror, the so-called instrument of Anagavstakis. I have long since wholly discarded it, not because it will not suffice for many cases, but because it will not suffice for all. I am convinced that difficult investigations will be most successfully conducted with an instrument that is, from daily use, perfectly familiar to the observer ; and for this reason I confine myself to a mirror that carries a magnifying lense behind it.

"With regard to the concave mirror, I should, until very recently, have echoed Dr. Zander's statement ; and should have agreed with the opinion of Mr. Hulke that Liebreich's small ophthalmoscope is the best and most convenient for the inverted image. Lately, however, the contrary testimony of very skilled observers has induced me to investigate the question with some care, and to compare the performance of different ophthalmoscopes upon eyes well adapted for testing them. I have come to the conclusion that my former preference was the result of habit, and that the best ophthalmoscope for the inverted image is, beyond all question, that of Coccus. It illuminates the fundus somewhat less brilliantly than Liebreich's, but more uniformly ; it is less interfered with by reflections of the mirror from the cornea, and by images of the flame upon the retina of the eye examined ; it affords better definitions, and permits the use of a higher magnifying power. Lastly, though inferior to Tishender's for the erect image, it yet shows it very fairly, and in a manner far superior to Liebreich's."—(pp. 72-75.)

XXIV.—*Die physiologische Optik. Eine Darstellung der Gesetze des Auges.* Von Dr. HERMANN SCHEFFLER. Erster Theil. Braunschweig, 1864.

Physiological Optics. An Exposition of the Laws governing the Eyes. By Dr. HERMANN SCHEFFLER. Part the First. Brunswick, 1824. pp. 467. 226 woodcuts.

Dr. Hermann Scheffler has supplied what has long been wanted by ophthalmic surgeons. In England the elementary treatise of Dr. Mackenzie is no longer adequate to the requirements of the day, and the article contributed by Helmholtz to Karsten's *Encyclopædia* is scarcely accessible, inasmuch as the publishers have long ago refused to sell singly the part of the *Encyclopædia* containing it. The volume before us contains a very complete and laborious account of the structure and functions of the several parts of the eye, and of its relations as an optical instrument to light and to the organism. The second part will chiefly have reference to physiological questions.

For such a book to be in all respects perfect, its author must possess very considerable attainments as a mathematician and physical philosopher, and also as a practical ophthalmic surgeon. In the latter respect Dr. Scheffler seems to be deficient, and hence to have fallen into occasional errors, which surgical readers will not find it difficult to correct. For example, at page 410, there is a

curious mistake about the action of Ruete's ophthalmoscope. The rays of light are described as pursuing a course that is optically possible, but which is not practically the true one; and the mistake is the more curious, since it is illustrated by two diagrams, and repeated with some emphasis in the text. The knowledge that Dr. Scheffler does possess is, however, far more important and far less common than that in which he is deficient; and we can very confidently recommend his laborious and learned treatise to all who desire to master the difficult subject of which it treats.

XXV.—*Traité Theorique et Pratique des Maladies de l'Oreille, et des Organes de l'Audition.* Par le Docteur J. P. BONNAFONT. 8vo, pp. 665. Paris, 1860.

Theoretical and Practical Treatise on the Diseases of the Ear, and of the Organs of Hearing. By Dr. J. P. BONNAFONT.

The diseases of the ear are a reproach to surgery. A very distinguished London surgeon once told us that he only knew two facts concerning them: the first being that people waited to be deaf before going to an "aurist;" the second, that, when they returned, they were always deafer than before. With some allowance for humorous exaggeration, this statement is probably not very wide of the truth; and it is certainly true that many practitioners regard aural maladies as being in their essential nature recondite and mysterious, and not amenable to the ordinary principles and common resources of our art. If Dr. Bonnafont, or the promoters of the new "*Archiv für Ohrenheilkunde*" can do anything to dispel this belief from the minds both of doctors and patients, they will confer no small boon upon humanity.

Dr. Bonnafont commences his work by a chapter on the general diagnosis of aural diseases, and the methods of examination followed by a chapter on general therapeutics. The next six chapters are devoted in succession to the diseases of the auricle, the meatus, the membrana tympani, the Eustachian tube, the tympanic cavity, and of the auditory nerves. The ninth chapter deals with deafness, the tenth with apparatus for assisting hearing, and the eleventh with congenital or infantile deafness and deaf mutism. The twelfth and concluding chapter discusses certain medico-psychological questions with reference both to the deaf and the blind.

A subject to which Dr. Bonnafont claims to have given very special attention is that of polypus of the auditory passages, and he believes this disease to be far more frequent and far more important than it is commonly supposed to be. He differs from Mr. Toynbee and M. Triquet, as indeed from most other writers on the subject, by insisting upon the frequent presence of polypi in young children; in whom, he declares, they are by no means uncommon, although usually overlooked. He draws a very alarming picture of

the symptoms that may suddenly arise from congestion of a soft polypus in the immediate vicinity of the tympanic membrane, symptoms which, without special knowledge of the subject, would be very likely to mislead practitioners. "The patient," he says:—

"If the polypus continue to enlarge in the direction of the tympanum, the patient will suffer from a series of symptoms due to compression of the tympanic membrane. Thus, there will be pain, usually very acute, at the bottom of the meatus, increased by mastication, yawning, cough, or deglutition: each of these movements forcing a volume of air along the Eustachian tube into the tympanic cavity (as I have often verified), and thus adding a pressure in a contrary direction to that of the polypus. The result will be an increase of pain, which the vicinity of the *facial nerve* (?) will cause to radiate over the head and face of the affected side."

"The patient, without suffering much headache, will have attacks of dimness of sight, of vertigo, and often of sickness. During the acute stage there will be also an uncertain gait, wavering like that of a half tipsy man. All these symptoms, which sometimes attain great intensity, will cease suddenly after an escape of blood that relieves the tension of the polypus; which, if its presence in the meatus be not revealed by some special symptom, may remain for months or years producing only some uneasiness, and the greater or less discharge and deafness that are its inevitable attendants. It would be easy to cite numerous examples of persons who have had polypi in their ears for many years, without the existence of the growths being even suspected by their medical attendants."

The first effect of an aural polypus, according to Dr. Bonnafont, is to produce more or less accidental deafness by mechanically obstructing the meatus. By its increase, it eventually produces organic changes in the neighbouring parts, ulceration of the lining membrane of the meatus, perforation of the tympanic membrane, loss of the ossicles, caries of the bony meatus or of the mastoid cells, leading to abscess and exfoliation, or the extension of inflammation to the membranes of the brain. In the meantime the polypus, by blocking up the external outlet, may cause foetid pus to be directed down the Eustachian tube and swallowed, and thus may produce nausea, vomiting, anorexia, and other symptoms of gastric derangement.

It fortunately happens that a disease so serious in its consequences cannot remain undetected if it be looked for. A glance into the ear with good illumination (a point much and justly dwelt upon) will discover a polypus if it be there. In some cases, however, where a polypus springs from the *membrana tympani* and spreads itself out in a mushroom shape, so as wholly to cover the membrane without projecting far into the meatus, the sight alone might leave its presence doubtful. Such a polypus, however, will receive from a fine probe a certain extent of rotatory movement upon its pedicle, and may, by this simple test, always be discovered with certainty.

Since a polypus at first produces only accidental deafness, then organic deafness more or less complete, and lastly, other changes that may soon imperil life, the only admissible treatment is its re-

moval, whenever detected, without unnecessary delay. When it springs from the side of the meatus, the surgeon may choose between evulsion, ligature, and excision; when it springs from the tympanic membrane, between ligature and excision only. Dr. Bonnafont usually prefers excision, although he practises all three methods, and gives very full and clear directions for the accomplishment of each, illustrated by figures of his instruments. For excision he uses a set of tiny knives, one straight, one concave-edged, and two curved nearly to a right angle on the flat, and cutting right and left; with these and a small double hook, all mounted upon suitable stems and handles, the excision of any polypus, however situated, may be effected. When the polypus is removed, by whatever method, he cauterises the remnant of its pedicle with a fine point of solid nitrate of silver, and repeats this application as often as circumstances may require.

Dr. Bonnafont's work is throughout clearly and pleasantly written; his views are plainly stated and well defended; and in his occasional differences from other aural surgeons, he treats them with due respect and courtesy. His treatise deserves, and will repay, a careful perusal.

XXVI.—*The Use of the Laryngoscope in Diseases of the Throat, with an Appendix on Rhinoscopy.* By MORELL MACKENZIE, M.D. Lond., M.R.C.P., Physician to the Dispensary for Diseases of the Throat. Hardwicke. 1865. 8vo, pp. 154.

Brief, yet with sufficient detail, clear, excellently illustrated, and (a not unimportant consideration) well printed, this book forms a capital guide to the art of laryngoscopy. The laryngoscope and the ophthalmoscope must ultimately come into as common and constant use as the stethoscope. The discovery of each of these instruments forms an epoch in practical medicine. To Dr. Babington, Dr. Mackenzie justly assigns the honour of being the inventor of the laryngoscope. He published an account of his invention in March, 1829. "The only difference between Dr. Babington's laryngoscope and the one now in general use is," says our author, "that whilst in the latter the light is thrown into the larynx (or rather on to the laryngeal mirror) by a circular mirror attached to the head of the operator, in the former the illumination was effected by a mirror held in the operator's hand." But the development of laryngoscopy was not contemporaneous with the invention of a laryngoscope. The former was due to Professor Czermak of Pesth, and dates from 1857.

The following observations show the spirit in which Dr. Mackenzie's book has been written:—"I would remark, that, with very few, and very simple appliances, the most satisfactory results may be accomplished, not only in diagnosis but in the treatment of laryngeal disease. I have already observed that many of the most valuable laryngoscopic investigations have been made with a common

moderator lamp, and I would call attention to the fact, that laryngeal growths have been removed with forceps of the most simple description. The forceps which Dr. Fauvel has several times used with success has no complicated mechanism, and the instrument used in Dr. Russell's case was "an ordinary pair of curved forceps." Those who do not intend to take up the subject from a special point of view, but merely wish to use the laryngoscope in general practice, will do well to provide themselves with a reflector, a couple of laryngeal mirrors, a light-concentrator, (which can be used with different kinds of lamps), a few laryngeal brushes, and my laryngeal galvanizer. A very large proportion of laryngeal diseases can be treated with the brush alone, and obstinate cases of functional aphonia cannot resist the internal application of galvanism. An additional recommendation to these instruments is, that even when employed injudiciously or ineffectively they are not likely to do any harm. It is only after the eye and the hand have had much practice in applying remedies to the larynx, that the lancet can be used with safety or the forceps with effect. In conclusion, I feel it a duty to remark, with Dr. Johnson, upon the possibility that the larynx may get too much of local treatment. The laryngoscope has brought this organ so completely within our reach, that we are all exposed to the temptation of being too meddling. If we can avoid the error to which I have here alluded, the introduction of the laryngoscope will be an unmixed good both to ourselves and to our patients, and it will soon be acknowledged to be one of the most valuable additions that have ever been made to our diagnosis and treatment." (p. 137.)

XXVII.—*Chloroform: its Action and Administration.* A Handbook. By A. E. SANSOM, M.B. Lond. London: Churchill. 1864.

The aim of this volume is to give a full and complete résumé of the present state of our knowledge respecting chloroform and its effects. From his researches, the author concludes that:—"Though chloroform has in many instances caused death, it has not the fatal power which might at first sight appear. That, excluding the various chances which all chloroformed and not chloroformed must submit to, by utilizing our increasing knowledge and exercising an increased care, we may deprive it of nearly all its terrors. That by the immense preponderance of its influence for good it has been a direct conservator of human life." (p. 17.)

The effects of chloroform, it is now well known, are not manifested at once; a period of from three to four minutes must at least elapse before they show themselves, the first symptoms being those of exhilaration, attended with an increase of the heart's action, whilst the anæsthetic, or rather narcotic, effects are not produced till after a minute or two. The period of complete muscular relaxation is preceded "by a muscular tremor, a struggling, slight, if the chloroform be administered slowly; occasionally violent, if a large

dose be given. The author considers this tremor as the best sign that the patient is sufficiently under the influence of the drug, and deprecates the pushing of the inhalation to such a degree as to bring on insensibility of the eyelids. The great source of danger in chloroform inhalations is now universally acknowledged to be its insufficient dilution with atmospheric air. According to the report of the Chloroform Committee of the Medico-Chirurgical Society, three and a half per cent. should be the average, and four and a half per cent. the maximum of chloroform contained in the air inhaled. In order to insure this all-important object, various inhalers have been devised, of which Mr. Clover's seems as yet to answer the purpose best. Dr. Sansom, however, suggests another of his own modification, which is said to have one advantage at least over Mr. Clover's apparatus—that of being less cumbrous. The interest which attaches to the subject of chloroform is so very great that this little hand-book is sure to find numerous readers.

XXVIII.—*Etude sur les Kystes de l'Ovaire et l'Ovariectomie.*

By Dr. VEGAS. Paris: Louis Leclerc.

Memoir on Ovarian Cysts and Ovariectomy.

This is one of the few memoirs, which have been as yet published in France, on the important question of ovariectomy—an operation which has reflected so much credit on British surgery. The author devotes the first part of his essay to the consideration of the pathological anatomy of ovarian cysts, their mode of formation, the varieties which they present, and their diagnostic symptoms. In the second part, the operation is fully described in all its stages, and all the minute details which constitute such an important feature of the after-treatment, are wisely dwelt upon. As the result of his investigations, the author concludes that ovariectomy is as legitimate as any other capital operation, that it is not attended (as was at one time supposed) with so many risks, and that this is clearly proved by the statistics published by Clay, of Manchester, Baker Brown, Spencer Wells, and Tyler Smith.

XXIX.—*Essai sur le Traitement des Fistules Génito-urinaires chez la Femme.* By Dr. JOSÉ E. MONTEROS. Paris: L. Leclerc.

Memoir on the Treatment of Genito-urinary Fistula in Woman, &c.

This is a rather voluminous memoir in which the author passes in review all that has been written on the interesting question of the treatment of vesico-vaginal fistulæ. After describing the four principal methods of operating which have been recommended by eminent surgical authorities, he concludes in favour of Dr. Marion

Sims' method, as being the most successful. Perhaps the most important parts of the essay are the valuable tables appended to it, showing the results of operations performed according to the different methods. And last, though not least, are some very good lithographs illustrating the different stages of the operation as performed by Dr. Marion Sims—the attitude of the patient, the kind of suture, and the various instruments used, &c.

XXX.—*The Surgical Diseases of Children.* By THOMAS BRYANT, F.R.C.S., Assistant-Surgeon to Guy's Hospital, London: Churchill. 1863. 8vo, pp. 147.

This work consists of the Lettsomian Lectures delivered before the Medical Society of London in 1863. It touches upon the whole of the surgical diseases incident to childhood, and contains the results of Mr. Bryant's extensive experience in connexion with Guy's Hospital. We commend it to all surgical practitioners. In illustration of its contents, the observations on malformations of the anus and lower bowel, and on the diseases of joints, may be referred to. Mr. Bryant sums up his observations on the former subject as follows:—

"1. In all cases (with some rare exceptions), whether of imperforate anus, obstructed rectum, or misplaced anus, an exploratory operation in the normal anal position is perfectly justifiable; and it may be attempted with the fair hope that, in nearly half of such cases, primary success will be secured.

"2. Such exploratory operation, however, to be successful, must be conducted with great caution; and the exploratory puncture or incision is to be made upwards and backwards towards the sacrum.

"3. If these measures fail, or if from some peculiarity in the case they appear useless and unjustifiable, the intestine is to be opened in the groin; the lumbar or Collison's operation being quite inapplicable.

"4. In the inguinal operation, the right groin appears to possess advantages over the left; as the intestine is found with more certainty, and the benefits to be expected from the operation are equally great.

"5. The treatment of these does not terminate with the success of the primary operation; for constant dilatation of the artificial anus is a necessity to preserve life."

On the general treatment of diseases of joints in children, we quote the following excellent observations:—

"In the treatment of all cases of joint disease, whether in the child or adult, it is right that we should have almost unlimited confidence in the reparative powers of nature to effect a cure, as it can be only through such a feeling that the surgeon would be preserved from interfering in the process of recovery, and from lapsing into the practice of that great error—meddlesome surgery. In the treatment of diseases of the joints in children, the necessity to bear this conviction in mind is far more necessary than it is even in adults; for it would almost appear as if there is no amount of mischief to the articulation of a child which is not remediable by natural processes; and that during young life, every disorganized joint is capable of recovery, although, perhaps, it may be with ankylosis. I believe this

opinion to be strictly true, with one exception, and that is, when any necrosed bone is present to keep up the irritation and prevent the completion of the cure. Under any other circumstances, a disorganized joint appears capable of repair, and in the diseases of children the proof of the accuracy of these opinions is constantly brought before us. The only drawback, however, to their universal application depends much upon the condition of the general health; for it is in this that the grand fault always lies. The chronic inflammatory affections of an articulation are essentially diseases of debility; they are found in children who are naturally frail, or in others who have been brought into a cachectic condition by some other affection, such as fever, &c. Let these general conditions be improved, it is quite certain that the local affection will improve also; and that as the general health of the child becomes re-established, the local health of the articulation will also return. In the treatment of joint-diseases, the surgeon's duty lies, therefore, in the carrying out of simple principles. He must, first of all, look to the general health, and employ all hygienic and medical means to improve and sustain the powers of his patients—good food, good air, and such tonic remedies as appear applicable, being of primary importance; for without these, all local treatment will doubtless fail. Specific treatment, such as mercurial remedies, is rarely required; it would be better for such remedies to be altogether forgotten, than that they should be generally resorted to in chronic joint-affections. In some few cases they may be of service, but such cases are few and far between."

XXXI.—*La Médecine du Bon Sens. De l'emploi des Petits Moyens en Médecine et en Thérapeutique.* Par P. A. Piorry, Professeur de Clinique Médicale à la Faculté de Médecine de Paris, Chevalier de la Légion d'Honneur, Médecin de l'Hôpital de la Charité, &c., &c., &c. 12mo, pp. 523. Paris. 1864.

Common Sense in Medicine. The use of Small Things in Medicine and Therapeutics. By P. A. Piorry, &c., &c.

If Dr. Watson, or Dr. Copland, were to publish a treatise with a title similar to the above, and, almost literally, *de omnibus rebus*, it would fairly be taken as an attempt to supply the actual deficiencies of medical practitioners, and as the attempt, moreover, of a writer whose daily life must have taught him precisely what those deficiencies are. If we may take a similar view of the treatise issued by the venerable father of pleximetry—if we may regard it as containing a key to the kind of knowledge in which our brethren in France are, as a body, uninstructed—it is impossible to rise from its perusal without a very strong conviction that our methods of medical education must, for all the daily duties of our calling, be very far superior to theirs. The book before us is largely filled by precepts and suggestions that are good and true, but that are too obvious to be often promoted to the dignity of printed matter. In this country they form part of the unwritten law that guides the practice of students or midwives; and many of them might not inappropriately be set

out upon a broad sheet for suspension in cottages, under the auspices of the Ladies' Sanitary Association. Dr. Piorry, however, invests these trivialities with an absurdity not naturally their own. Nobody ever forgets or neglects them; but Dr. Piorry introduces them with trump and banner. In some cases he gravely disputes an imaginary claim to them on the part of England; and, in one remarkable instance, he seeks to appropriate, to France and to himself, the honour of having originated, in 1838, an expedient that was probably practised by the original proprietor of the Neanderthal skull—since, in this, as in many other instances, it is difficult at first to perceive how the specified precaution could be omitted. The brief preface, in which the author not only deploras the rampancy of quackery, but says that, *as a rule*, the honest practitioner must find, in his own consciousness of desert, the reward which the public will not extend to him, certainly lends colour to the belief that, in France, there must be some such radical defect in professional training as this book, if it be not entirely superfluous, assuredly indicates. The public, either in France or elsewhere, will seldom withhold their confidence from those who merit to receive it.

We opened Dr. Piorry's volume with the full expectation of drawing from it a fund of practical information of an important kind, readily available in the sick-room; and it is with much disappointment that we have recognised its real character, and discovered in its pages nothing to repay us for perusing them. A statement about the occasional utility of introducing some unctuous substance into the vagina during labour is, perhaps, a fair specimen of its general character. It concludes, however, by an appendix more ambitious than the volume, and containing suggestions, and a synoptical table, for the reform of medical nomenclature. Writers who desire to frame new words upon recognised principles, may possibly find these suggestions valuable.

XXXII.—*Applications de la Gymnastique à la guérison de quelques Maladies, avec des observations sur l'enseignement actuel de la Gymnastique.* Par NAPOLEON LAISNÉ, Professeur de Gymnastique à l'Hôpital des Enfants Malades, &c. Paris: Leclerc. 8vo, pp. 411. 1865.

The Cure of certain Diseases by Gymnastics, &c.

The application of gymnastics to the treatment of diseases demands greater attention in this country than it has yet received. There can be little doubt of the advantage which can often be derived by a regulated system of muscular exercises in certain forms of paralysis, some convulsive affections, and some abnormal deviations from the symmetry of the body. English medical men, however, have as a rule looked askance upon gymnastics as a therapeutical means. Unhappily, in this light, they have too commonly been presented in so dubious or unsatisfactory a form as to repel notice. The manner of

presentation has had much to answer for this result. The writers on the subject have not dealt with it in a fashion to inspire much confidence. M. Laisné has not escaped altogether from the inaptness which has distinguished too many of his predecessors. This book deals less with his subject than with himself. A careful and trustworthy account of his gymnastic experiences at the Hospital for Sick Children and the Salpêtrière would have been an interesting and perhaps useful addition to medical literature. But such an account is not forthcoming. Only one disease occupies a prominent place in M. Laisné's book, Chorea; and respecting this malady, he chiefly contents himself by republishing at length Dr. Blache's paper on its treatment by gymnastics, presented to the Academy of Medicine, and Dr. Bouvier's report on the subject. It is to be inferred from M. Laisné's statements, rather than directly gathered, that regulated gymnastics have been found useful by him in epilepsy. A systematic account of the treatment of this disorder by regulated exercises would unquestionably have commanded attention. As it stands, his observations are of no worth. For the rest, M. Laisné's book is not to be read without some instruction; but it is a work which would be eminently improved by Sidney Smith's rule of revision—the omission of two words out of every three.

XXXIII.—*A Letter on Corpulence*. Addressed to the Public, by WILLIAM BANTING. Third Edition (pamphlet), pp. 50. London: Harrison. 1864.

One of the most notable events of the day is Bantingism, or the rage to conquer corpulence by the adoption of the mode of diet by means of which Mr. Banting underwent a speedy metamorphosis from unwieldy stoutness to more seemly dimensions. Mr. Banting, according to his own account, had long been almost altogether overborne by the woes of obesity. He had done all he could to obtain relief, and he had lost all hope of success, when he was advised (by Mr. Harvey, of Soho Square) to adopt the diet in question. On the 6th of August, 1862, when this advice was given, his age was 66; his height, 5 feet 5 inches; his weight, 202 lbs., or 14 stone 6 lbs. On the 26th Aug., 1863, he had lost 46 lbs., or 3 stone 4 lbs. in weight, and 12½ inches in girth, and he had regained a degree of health and activity to which he had been a stranger for years. The diminution in weight was curiously gradual—in the first quarter of the year about 12 lbs., in the second quarter about 12 lbs., in the third quarter about 11 lbs., in the fourth quarter about 11 lbs. The last memorandum of weight was on the 12th of September, 1863, and at that time there had been no change for upwards of a fortnight. No wonder, therefore, that a fact like this should make a deep impression on the public mind. No doubt, also, that the knowledge of this fact is calculated to do much good, by pointing out certain common errors in diet, in a very striking manner. At the same time it is certain

that this good is not altogether unmixed with evil, and it is this thought which suggests the remarks which follow.

To abstain as much as possible from bread, butter, milk, sugar, beer, and potatoes—articles containing starch and saccharine matter, and “tending to create fat,” and to take lean animal food, and claret, sherry, or Madeira, and tea liberally, is the rule which must be observed by the followers of Mr. Banting. Speaking of his own particular dietary, this gentleman says:—

“For breakfast, I take four or five ounces of beef, mutton, kidney, broiled fish, bacon, or cold meat of any kind, except pork; a large cup of tea (without milk or sugar), a little biscuit, or an ounce of dry toast.

“For dinner, five or six ounces of any fish except salmon, any meat except pork, any vegetable except potatoes, one ounce of dry toast, fruit out of a pudding, any kind of poultry or game, and two or three glasses of good claret, sherry, or Madeira—champagne, port, and beer forbidden.

“For tea, two or three ounces of fruit, a rusk or two, and a cup of tea without milk or sugar.

“For supper, three or four ounces of meat or fish, similar to dinner, with a glass or two of claret.

“For night cup, if required, a tumbler of grog (gin, whisky, or brandy, without sugar), or a glass or two of claret or sherry.”

Upon getting up in the morning Mr. Banting takes a “tablespoonful of a special cordial, which may be called the Balm of Life,” but this practice, in all probability, is one which may be continued or omitted without any material result, and therefore it is not necessary to go out of the way to consider it.

What, then, of this particular form of diet? Does it secure the proper nourishment of the body? Does it secure the maintenance of the proper warmth of the body? A properly regulated diet ought to secure both these ends; and it is difficult to believe that either of the ends is secured by the diet in question.

It is difficult to believe that the diet in question maintains the warmth of the body in the best manner. The heat is maintained by the combustion of the carbon and hydrogen of the food, and there are certain articles of food, as starch, sugar, and fat, which seem to be chiefly intended to supply this carbon and hydrogen,—to act, in fact, as fuel. But these are the articles which, according to the code of Bantingism, are to be as much avoided as possible. The heat is also maintained by the combustion of carbon and hydrogen of those articles of food (as fibrin, albumen, and casein) which have the power of nourishing the tissues of the body—*food proper* in place of mere fuel, though fuel also, and it is in this source chiefly that Bantingists find that carbon and hydrogen keep themselves warm. They are carnivorous animals to all intents and purposes, and the question is—can they afford to live in this manner? Nor is this a difficult question to answer. The fibrin, albumen, and casein from which they have, at one and the same time, to get food and fuel, contain nitrogen as well as carbon, hydrogen and oxygen. For each equivalent of carbon and hydrogen which is burnt as fuel, there is some nitrogen to be disposed of: and what becomes of the nitrogen? It becomes urea, and, in certain cases, uric acid, and escapes by the

kidneys. And hence a doubt. For if the kidneys are not capable of doing their work well, the elimination of urea, and it may be of uric acid from the blood, becomes imperfect, and the oxygen is left in the state which is either one of gout, or one which may readily become gouty. Hence it follows that there must be an extraordinary energy of the system generally and of the kidney apparatus especially, if the person can keep himself warm in a safe manner by eating lean meat to the extent which is prescribed by Bantingism.

Nor, as it would seem, is it right to look upon starch, and sugar, and fat, in the light in which they are looked upon by Bantingists. These bodies are the natural means of maintaining the warmth of the body. They contain no nitrogen, and therefore there are no urates and urea resulting from their combustion, and contaminating the system if the kidney is not up to its work. They consist in the main of carbon and hydrogen, and the carbonic acid and water which result from the combustion of these bodies escape in the main readily by the lungs and skin. But it is objected to these bodies that they form fat—that the unburnt portions, those which are not breathed away as carbonic acid and water, become fat. This is the objection, and up to a certain point it is sound. In excess one or other of them does form fat under these circumstances, but is this the case with the three? It may be questioned, for example, whether starch has this fattening power. The Irish peasantry live in the main on potato, which is chiefly starch, but certainly they are not stout as a people. The French peasant is a great bread-eater, and bread is rich in starch; but the French peasants are not stout as a class. The Hindoo poor live chiefly on rice, which is richer in starch than potato even, and the Hindoo poor are notoriously lean. It is the Hindoo of the better orders who likes large quantities of ghee as well as starch, and who is often very stout. It may also be questioned whether sugar is so fattening as it is assumed to be. The negroes during the sugar harvest are said to become sleek and fat, but it is not quite easy to ascribe this change to the eating of sugar simply; for at this time their diet is greatly improved—they have more pork, which is in the main fat; and they have more Indian corn, which is rich in oil. In a word, it may be wrong to confound fat with starch and sugar in its uses in their economy. At the same time, it is evident from the experience of polar regions that fat is the most important food for maintaining the animal heat, and therefore, in this point of view—the producing of heat, that is to say—it may be wrong to exclude fat and oily matters from the diet. No doubt there are cases where fat is especially unsuitable, as in persons whose livers are very “sluggish;” no doubt, also, there may be cases in which sugar is not most suitable as respiratory food—cases, for instance, of a rheumatic tendency, where it may be suspected that sugar may aggravate this tendency by giving rise to the formation of lactic acid; but these cases are exceptional rather than otherwise. Indeed, unless some better arguments are forthcoming than those which are supplied to the Bantingists, it will be well to look upon milk, eggs, and bread, all of which are proscribed, as at once showing that oily matters, sugar, starch, are proper elements

in the food, and that they must be present in a certain proportion to keep up the natural warmth of the system. Thus: in 100 parts of milk the casein is only 5, but the butter is 3·5, and the sugar 4·5. In the yolk of eggs, in 100 parts the albumen is only 17·47, whereas the oil or fat is 28·75. And in wheat flour the gluten and albumen is only 14·6, whereas the starch is 59·7, the sugar 5·5 and the fat 1·2. With facts like these, then, it is difficult to believe that starch, sugar, and fat have not a necessary function to fulfil in the economy, and there may be a grave blunder in excluding them as is done in the diet under consideration.

But if there is reason to believe that Bantingism does not provide in the best manner for the maintenance of the natural heat of the body, what of the nutrition of the body? Does it secure this end in a more satisfactory manner? This is scarcely to be expected, for it is quite plain that excess of animal food, by loading the blood with urea and its products, must sooner or later interfere with nutrition in a very serious manner. Nor is this difficulty a mere matter of theory.

The diet in Bantingism is practically the diet which is adopted by those who are in training either for the ring or the boat-race. It is even this diet in excess; thus Mr. Banting allows himself from thirteen to fourteen ounces of lean meat a day—considerably more, that is to say, than is taken by an average sized man in process of active training. Now it is well known that the process of training can only be kept up for a certain time—three or four months is a long time, upon the average. At the end of this time, there is a break-down—a break-down which often takes a gouty character, the muscles becoming stiff, painful, and so on. And what is known to take place in the process of training is precisely what takes place, to the knowledge of the writer, in many persons who have given Bantingism a trial for a certain time. At any rate, we can say, without any hesitation, that we have seen several Bantingists who have soon broken down egregiously. Some of them have been gouty, others have been curiously wanting in energy of mind and body, and we have come to the conclusion that they could not continue the experiment in which they were engaged with impunity.

Nor can there be much difficulty in accounting for this change for the worse. Excess of urea and water in the blood, will not only account for this gouty disposition, but also for this wanting energy. For with a state of impure blood there must be a corresponding want of energy. Moreover, it is possible that oily and fatty matter may be as necessary to the proper nourishment of nerve-tissue as flesh is to the proper nourishment of muscle and that the exclusion of fat from the diet in training may be one reason why persons in training (Heenan for example) have been so curiously wanting in power of endurance. It may be, indeed, that the muscular power of these men has been kept up by their food, and their nerve power starved for want of fatty and oily matter.

Exceptions, moreover, may be taken to Mr. Banting's rules for the use of alcoholic drinks; and it may be questioned whether these are not much too lax in the matter of quantity. At any rate it is certain

that persons in training, who are practically moderate Bantingists, and who, one would think, are less likely to get out of order than Bantingists proper, very soon get out of condition unless they are very abstemious in the matter of alcoholic drinks. Nor is there much difficulty in finding good chemical reasons for this fact.

With respect to tea, it is more difficult to give an opinion. Probably, however, it may be antidotal to the excess of alcoholic drinks, and in this way it may do good.

Dr. Lankester says—"Unless a due proportion is maintained between the heat-givers and the flesh-formers, disease and death may be the consequence," and we venture to say that this proportion is not preserved in the diet of the Bantingists. We do not venture to describe what is the proper proportion between these two groups, or how much the vegetable world, and how much the animal world should be placed under contribution; but at the same time we incline to think that in the great majority of cases it is safer to overtax the former than the latter. There must be a difference between different persons and between the same persons at different times of life, some persons inclining to the type of vegetable feeders rather than to the type of animal feeders—all persons, perhaps, at some time. It is foolish to overlook the fact that albumen, casein, and fibrin exist ready formed in the vegetable world, and that there are vegetable-feeding animals who are best nourished on vegetable food. At the same time, while making these strictures, we fully allow that Mr. Banting has done good service by showing how much unwieldy corpulence may be kept under control by diet, even though that diet be open to many objections, and needing revision even in its fundamental features.

APPENDIX.

*On the Physiology of the Brain and Nervous System: a
Lecture delivered before the College of Physicians in
Ireland, February 2, 1865.*

By DR. BROWN-SEQUARD.

(*Dublin Medical Press*, February 10, 1865.)

[It is not our custom to reprint articles *in extenso*, but in the present instance there is good reason for striking out into a new course. All that is here said, indeed, is deserving of attention; and very few, if any, superfluous words are used in saying what is said, and therefore it is alike difficult to omit or condense.]

This lecture, I am afraid, will have many faults. Laying aside the principal one, of which I do not mean to speak, it has to treat of so many topics, that the variety will itself be a great obstacle to the comprehension of many of the views which will be put forward. I shall, however, endeavour, while putting forward as many facts as possible, to condense them, to accommodate them one with another, and to establish some tie, some union between them, so that they may, as far as practicable, form one connected and consistent whole. That whole will have for its principal object to show the immense importance—to the physiologist, of the study of morbid cases in the human species—and to the practitioner, of the study of physiology, and especially of that branch of physiology which is founded upon experiments on animals. Indeed, a complete revolution has been made in the practice of medicine in this century from the study of effects observed upon animals, and if, together with the light thereby thrown on morbid cases in our own species, we make an appeal not only to physiology—and experimental philosophy particularly—but if we also try to throw the light on normal anatomy; and especially the anatomy of the nervous centres, and of the nerves at the base of the brain, we find that to understand the symptoms of a large class of pathological cases in our own species is almost as easy as to read the alphabet. Indeed, a great many of the most complicated, the most obscure, and the most unintelligible cases of nervous complaints are as easily understood as the simplest case of bronchitis or any slight inflammation, if we have the light which experimental philosophy and the anatomy of the base of the brain now give us.

Suppose, for instance, a patient comes to us with paralysis—a per-

fectly and absolutely complete paralysis of one-half of the body, from the neck downwards. Suppose that, in addition to this paralysis of motion (say in the right half of the body), he has also on the same side extreme hyperæsthesia, or increase of sensibility in all those parts which are struck with paralysis of motion. Let us suppose that we find not only extreme sensibility to touch, which we may measure with the compasses, but also an extreme sensibility to tickling, which is a sensibility quite distinct from the other. Suppose we find, besides all this, that the sensibility to a prick or a pinch—in fact, the sensibility to painful impressions—likewise the sensibility to changes of temperature, to cold and heat, is also immensely increased. Thus, you have these four kinds of sensibility, which, I repeat, are absolutely distinct one from the other, all considerably increased in that limb apparently dead, as it has not the least power of motion. Suppose besides, that in the same limb we find the temperature considerably increased, and that the circulation there is more energetic, not more rapid of course, because it is the same heart that propels the blood there as elsewhere, but fuller, there is decidedly more blood there than elsewhere in the body.

All these symptoms, mark, are observable on *one* side of the body—the right; if we now contrast them with what we observe in the limbs on the opposite side, they acquire still greater interest. If we examine the *left* side of the body, we find an absolutely reverse condition. We find that all those various species of sensations of which I have spoken are lost, completely and absolutely lost, upon the left side. We find there is also on the left side—in opposition to what exists on the right—a complete power of movement, not the least diminution of the power of the will. Suppose, going further, we examine into the condition of the fifth kind of sensibility existing in the limbs (admitting that there are only five species of sensibility), if we examine into the sensibility that exists in the muscles which help us to direct our movements, we find the muscular sense remains perfect in those limbs which have lost the other kinds of sensibility. There is on the *left* side, therefore, a complete anæsthesia of the *first four* kinds of sensibility of which I have spoken, combined with a *persistence of the muscular sense and persistence of voluntary motion*; while on the *right* side the condition is exactly the reverse. But what about the degree of heat in the limbs on the left side, and what about the circulation? On the left side there is not only a much *inferior* degree of heat than on the right side, but there is also an actual diminution of heat, if you compare those parts with what they were in their normal condition; in other words, there is an *absolute* diminution of heat on the left side,—not simply a relative diminution; so that while on the *right* side the temperature of the body has increased, on the *left* it has diminished, and similarly with regard to the circulation.

These features are striking enough, yet there are many others in the same individual upon which I cannot now dwell, but which are fully as interesting and equally difficult to understand by a man who is not *au courant* with the present state of physiological science.

In the *face* on the side of the injury (admitting that an injury is the cause of these symptoms), there is an increased heat, an increased sensibility, a contraction of the pupil, and a degree of occlusion of the eyelids, so that the two eyes, if you look at them at the same time while open, are quite different one from the other; the eye on the side of the hyperæsthesia and of the increased heat is smaller, and the opening of the eyelids smaller than on the other side.

All these facts we produce in animals very easily, and it has been my good fortune (in some respects) to find many such cases in our own species; one of the most striking of them was seen with me by my dear and talented friend Dr. Robert Macdonnell, in which the symptoms upon which I have been dwelling were as marked as possible.

Now, what was the injury which produced all these remarkable effects? It was simply a division of the spinal cord, not simply of the half of that organ in the neck, but of the entire half—i.e., the posterior column, the lateral column, the anterior column, and the grey matter of one side, had been divided completely. Owing to that injury all those symptoms existed.

Now, I put it to you, if any physician at the beginning of this century, not knowing the present state of our knowledge in physiology, no matter how learned and how able in other respects, had such a living problem been presented to him, he would most certainly have been at a loss to understand the case. Nay, more, he would not *see* the case as I have described it; he would not recognise the existence of those symptoms; he would probably commit the error committed by the great French surgeon who had had such a case, but who never discovered that the sensibility was lost on the side still under the power of the will until the nurse told him of it.

From these facts you may see the importance of a thorough knowledge of physiology. The physiologist can have no difficulty in understanding such a case; for when he knows that the spinal cord is the organ conveying the orders of the will to the muscles, that the nervous fibres serving for voluntary movements pass from each side of the spinal cord, so that the fibres which serve for the movements of limbs on the *right* side pass to the *right* side of the cord, and those serving for movements of limbs on the *left* side pass to the *left* side of the cord, it is quite evident that such a division of the cord as I have described will produce loss of motion on one side, and not on the other. On the other hand, the sensitive nerve-fibres which serve to the first four kinds of sensibilities I speak of, pass into the spinal cord in such a manner as to go to the *other side* of that organ, so that the nerve-fibres of sensibility in my *right arm* and *right leg* pass into the *left side* of the spinal column, and *vice versa*, and thus a division of the cord produces loss of sensibility on the opposite side of the body to that of the injury.

Equally clear is the explanation as regards the increase of heat in the limbs on the side of the injury. The nerves of bloodvessels pass into the spinal cord on the side corresponding to that of the limbs into which they go, just as with the nerves of voluntary

movement; so that a division of the spinal cord in or below the neck on the *right* side produces paralysis of the nerves of bloodvessels in the right side of the body, in consequence of which paralysis there is an increase of the impulses of the heart everywhere on that side, causing a greater efflux of blood and increased heat, and (as a consequence of the increased heat) in a measure also the increased sensibility of which I have spoken, the hyperæsthesia of the four kinds of sensation.

I cannot dwell further on this class of cases; enough has been said to show how much light physiology can throw on symptoms which certainly would have been most obscure (to say the least) to even the most eminent men of the beginning of this century, who did not know the physiological facts which have been since discovered.

I shall now produce another case.

Let us suppose a man has sustained an injury, not of the spinal column, but of the medulla oblongata above the decussation of the anterior pyramids,—not such an injury as would destroy life at once, not such a tumour or morbid alteration as would prove immediately fatal; but suppose an injury of sufficient extent to produce decided symptoms, though not enough to cause immediate death.

Now in this case you would have all the features which I have described in the former case, but with this difference, that as the anterior pyramids decussate below the injury, an injury on the right side would strike the fibres of voluntary motion belonging to that side going up to make their decussation before they passed from the right side into the left side, and it would also strike after their decussation those fibres of voluntary movement that have come from the left side. Thus, therefore, there would be in this case paralysis of movement on *both* sides of the body. As regards the state of the circulation and heat, and as regards the hyperæsthesia and anæsthesia, everything would be similar in this case to the former.

Let us now suppose the injury is a little higher up, and we shall find other striking differences.

Suppose a patient comes to you with paralysis of the external rectus of the eye—suppose on the right side—the *face* is paralyzed on the same side; there is also anæsthesia of the *face* on the right side; but in the left side it is not the face, but the *body*, that is affected with paralysis, both of sensibility and motion.

Here, then, is a case absolutely distinct from both the others—a case in which there is loss of motion and loss of sensibility in the *face* on the right side, and in the *body* at the opposite side. I cannot dwell at length on the other features of the case, but I must not pass from it without noticing a most striking feature. You will often find in similar cases that the tongue is perfectly free,—there is no loss of movement at all in that organ. You will find the facial paralysis is just the same kind of paralysis that exists when the facial nerve outside the cranium has been injured, *i.e.*, that the muscles which give expression to the face and also the orbit of the eye are paralyzed. This case, therefore, is quite distinct from cases of hemiplegia. As you are aware, in paralysis due to disease of

the brain you will find the face paralyzed on the same side as that in which the body is paralyzed, the orbicularis is not paralyzed, whilst the tongue is almost always somewhat paralyzed. In the class of cases I am now submitting to your notice the distinction is characteristic and striking. The grimaces which the patient makes, owing to the paralysis of the face, take place on the side of the paralysis, instead of on the other side, because the paralyzed side of the face is the opposite to that which is usually paralyzed. Besides all this, you will find that the sense of *taste* is altered on the side on which the face is anæsthetic. You will find further that the patient is in a state of considerable emotion; he will shed tears easily, he will gape frequently, and while gaping there frequently will be a sudden jerk of the paralyzed limbs. There is also generally considerable giddiness and tendency to vomit. I mean now only to put forward the principal symptoms.

Now, I ask, what is the explanation of this case? Do you think the most eminent man of the beginning of this century, not knowing the science of physiology and anatomy as we now know it, could have understood this case? Certainly not. Therefore, as you may perceive, physiology and anatomy are immense helps in the diagnosis of disease.

Now, the set of symptoms I have last described belong to a case of disease of the pons Varolii, striking at the same time the roots of the trigeminal and of the facial nerves before they have made their decussation, which is at the lower part of the pons Varolii, and striking also the sixth pair of nerves before it has made its decussation, producing paralysis of the side of the injury, just as much as if the cause existed in the nerve itself.

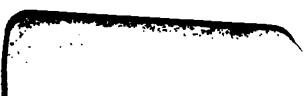
You must not, however, think that an alteration of the pons Varolii will in all cases produce all these effects. If the alteration takes place a little higher up than the middle of the organ, striking at the place where the facial nerve and trigeminal nerve cross, you will have these effects. The two sides of the face will be paralyzed, both in sensibility and motion; the two sides, as regards the action of the external recti of the eye, and also the sense of *taste*, will be paralyzed, while the paralysis in the body will be only on one side. It is quite sufficient, in order to understand this case, to keep in view what the nerves do when they reach the pons Varolii. When the injury strikes the pons Varolii *above* the decussation, you have, as regards the portions of the face and body which are paralyzed, the same effects as are observable in most cases of brain disease—that is, the paralysis of the face will be on the same side as the paralysis of the body; if, on the other hand, the injury strikes the pons Varolii *below* the decussation, the contrary effects are perceived.

The question, however, remains, how can you know when the disease is in the pons Varolii, if you lose that peculiar symptom] of the sides as regards paralysis of the face and body? In the first place, if the injury is one producing anæsthesia, you will find in the beginning of the affection, if the injury is in the pons Varolii, a most important symptom—viz., a great and extreme coldness of

that side of the body which is about to become paralyzed; in fact, just the reverse of what will occur when the paralysis is complete. I remember, on one occasion, my friend, Dr. Bright (who, like almost all great men, is extremely modest), did me the great honour of asking my advice upon a case which had come before him at the College of Surgeons. The symptoms in that case were—besides the extreme coldness already mentioned in one-half of the body—tingling in the fingers, very slight ptosis of the external rectus, some jerks in the muscles of the face, on the opposite side to that of the injury—a symptom you do not find when the injury is higher than the pons Varolii; also some sensations of tickling in the face—another symptom you do not find in cases where the disease is higher up than the pons; in fact, there were symptoms which, to one who was then more of a physiologist than a practitioner, as it is since that time I have seen more patients, were new in the human species; but I had no hesitation, simply from the teachings of physiology, in stating it was that kind of case which I have just pointed out; and so it proved, as it gradually and successively presented all the symptoms I have mentioned of disease in the base of the brain. Not having had the advantage of making an autopsy of the case, you may, perhaps, think me very presumptuous in holding that I had made a certain diagnosis while knowing nothing but the symptoms; but really with this class of cases doubt is impossible when the symptoms are combined, forming a group so defined and distinct that there is an absolute certainty even during life as to the cause. It is not so when the disease goes higher up in the brain. We are then at a loss, and it is extremely difficult to say whether there is organic disease, or merely a temporary disorder of the circulation; but if the symptoms I have pointed out are, all of them, present, the case is absolutely free from doubt or uncertainty.

I pass on to another kind of hemiplegia. There is one kind of hemiplegia absolutely distinct from all these kinds. Suppose a patient comes to you with some slight stiffness and tendency to throw his limbs on one side; there is not a great paralysis, but rather a decided weakness, and hardly any loss of sensibility at one side of the body. He complains also of noises in the ear on the same side, of feeling extremely giddy, and having a tendency sometimes to revolve and turn round upon himself. He sometimes reels as if he were intoxicated; he very frequently cannot walk straight forward; sometimes he has also very great hyperæsthesia to sounds; he has also at times a sudden tendency to fall down; it seems to him that he cannot keep up, and that he must fall, also that if he takes hold of something he will keep up.

This kind of case is, indeed, the most instructive of all kinds of hemiplegia. I have now collected more than twenty-two such cases—not all seen by me, but a large proportion of them. According to the autopsy made in a number of these cases, they are simply cases of reflex paralysis; they are not paralysis owing to the destruction, the alteration, or the section, in fact, to the interruption the conductors of voluntary motion. They are absolutely distinct



from the paralysis which is due to the fact that the conductor between the organ of the will and the muscle is interrupted—they are produced by quite another mechanism. In this class of paralysis there is disease, either of the lower part of the brain, at the base of which lies the fifth pair of nerves, or near the place of entrance of the auditory nerve. There is in this case not a destruction but a pressure (and not a very considerable pressure) on the cerebelli, a small part of the pons Varolii, and the medulla oblongata. Place a tumour there, which has encroached slightly and gradually on the neighbouring parts to those I have named, and these symptoms will appear.

But let the injury go further, then the paralysis on one side of the body—viz., the paralysis on the side where the injury existed—will disappear; yet the injury to the base of the brain is greater now than it was in the former case; but from the moment that a real disorganization has taken place in the base of the brain the symptoms which existed at first disappear, and the paralysis passes from the right side, where it existed at first, to the left side, the tumour still continuing at the right side.

I regret I cannot, owing to the limited time at my command, explain the cause of this more at length, but I will endeavour to do so in a few words.

In the first case the same condition exists as where an injury exists on a nerve anywhere in the body, producing paralysis. Acting upon the brain, it produces an alteration of some kind which the microscope does not detect, and by a reflex action produces paralysis. But why is it that in the second case we find the paralysis disappears at that side? The explanation is, that the part which in the first case was irritated has now been destroyed, and hence there is no more irritation, and paralysis consequently ceases on that side, but it goes to the other side of the body; because in the pons Varolii and medulla oblongata there are conductors of voluntary motion above their decussation passing to go up to the brain. Hence if an injury exists such as to destroy some of these conductors, paralysis will occur on the other side; and it disappears on the first side, owing to the fact that the part has been destroyed and there is no longer a pressure and irritation as at first.

I intended bringing forward a great many other types of hemiplegia to show what physiology can do to explain these cases; but I am compelled to be brief. I, however, must mention one other kind of hemiplegia—that due to hæmorrhage in the cerebellum. In this case there are features which have, most of them, been found in experiments on animals, and which, if rightly read, will lead to accurate diagnosis. One is, there will be vomiting—this is a constant symptom of hæmorrhage into the cerebellum. There will also be hyperæsthesia in some parts of the body—not the whole, nor even the half, but in some parts. There will also be amaurosis, not from pressure on the tuberculi quadrigemini, but due to reflex action, as the disease in most cases does not press upon the tuberculi quadrigemini. That this is so, appears still more certain if we take into account what occurs in many of these cases of amaurosis: we may

have amaurosis of the left eye alone, of the right eye alone, or of both eyes; still more, we may have amaurosis passing from one eye to the other alternately, showing, in fact, that there is no persistency or uniformity of action in the production of amaurosis in these cases.

There is another kind of hemiplegia as to which I must say a few words; it is that kind which is due to a lesion of the anterior lobes of the brain. Phrenologists, we know, have regarded the anterior lobes of the brain as organs of speech, but there are many cases—Dr. Stokes mentioned to me a very striking one a few days ago—in which a destruction of these lobes took place without any loss of speech. But the question (and it is an interesting one) arises, what creates the loss of speech when such loss exists? As regards that, I shall have in a moment or two to point out how great a variety of symptoms may be produced by a lesion of almost any part whatever of the brain. This loss of speech I hold to be a mere reflex phenomenon; and of this we have a proof in the fact that it will vary very much, even in the same patient, according to circumstances which physiology has as yet been unable to detect, but certainly with the lesion still continuing. The facts at all events prove that destruction of the anterior nerve is not the direct cause of loss of speech—it is one of the instances of reflex phenomenon. It is worthy of remark that the loss of speech is usually unaccompanied by any difficulty of movement in the tongue. There is perfect freedom of motion in the tongue, and the defect of speech arises from the patient being unable to express his thoughts not only by speech, but even by signs or writing; it is a paralysis of the “organ of expression of thoughts.” The patient may, notwithstanding, remain very intelligent. I some time since met a case—it was that of a clergyman—who was a remarkable instance of this. He had not absolutely lost his speech, for he pronounced words very distinctly, but they were words possessing no meaning whatever. He was likewise unable to write, or even to express his thoughts by signs. When asked, for example, to express “yes” by lifting one finger, and “no” by two fingers, he was unable to do it, although he appeared extremely intelligent.

I pass on to notice another group of facts, showing the importance of a knowledge of physiology in the diagnosis of disease. There is one form of disease to the discovery of which I have been led by experiments on animals, and which I must mention. Patients may come to you complaining of pain in the back, of a pricking sensation in both arms, with some degree of itching, burning, or some strange sensation of cold and heat alternately in the skin of both their limbs. You may find some strange forms of skin affection different from those which you have usually to deal with when they are not due to nervous disease. You may find also some degree of weakness in the two upper limbs, jerking in these limbs, sometimes also a great stiffness in some of the vessels, and they are tender under pressure. If you do not pay attention to the state of the spine—if you do not know the exact physiological meaning of all these symptoms—you will perhaps be led to suppose that there is some local affection

—rheumatism, if you like—of the arms. You, perhaps, will think it very strange that heat and cold can coexist in the same organs. You are surprised you can find no description of such a disease in books. Yet the explanation is most simple: it depends altogether on an inflammation of the nerves at their exit from the spine in the lower part of the cervical region. There is in the spine sometimes local meningitis. The whole thing arises from irritation of motor and sensitive nerves, and especially irritation in the nerves of blood-vessels—this it is which produces all these symptoms. If, then, applying your physiological knowledge, you arrive at the true character of the injury, you may, by a certain mode of treatment, which presents no great difficulty and causes no great pain, cure, or all events greatly mitigate the disease. I have met many cases of the kind, and with the exception of one, which I saw in London in conjunction with Dr. Adams, and which terminated in death, all the cases have been either cured completely or more or less ameliorated. The treatment consists in the most active blistering of the spine in the region of the injury, also in the application of dry cupping all along the lines of blister. Injections of narcotics have also been resorted to, but the principal treatment consists of the application of blisters to the spine. Internally I have sometimes employed iodide of potassium, but what share it had in the cure I do not know.

I intended to bring forward other cases of reflex paralysis of the lower limbs and from inflammatory softening of the spinal cord, but time does not allow of it. I will simply say, that physiology has demonstrated these most important facts—that the spinal cord in its central part, which is decidedly insensible in its normal state, will become exquisitely sensitive under the influence of inflammation; and when sensitive, it will give rise to all those strange sensations complained of by patients attacked with myelitis or great congestion in the grey matter. When there is considerable congestion of the grey matter, or, still more, when there is inflammation, we have these symptoms, which are also the effects of irritation of the motor nerves—viz., jerks, tremblings, convulsions, contractions in the muscles, etc. All these symptoms are due to a special change in the condition of the spinal cord; they cannot be produced without congestion or inflammation; they are essential to these two diseases, and if you do not find at least a part of them you may be convinced that the spinal column is free from congestion, and free still more from myelitis.

I proceed to notice two kinds of cases of fracture of the spine absolutely distinct one from the other, though the injury in both is in the neck.

Two patients are brought to you having sustained a fracture in the cervical region. One of them is almost pulseless, extremely cold, covered with a clammy perspiration, his limbs lie loose and dead, there is no contraction, no rigidity, his breathing faint. If you bleed him, you will find the venous blood red like arterial blood, flows out, not with a great impulse, for the impulse of the heart is extremely weak and almost in a state of syncope; but still the blood does not flow in the same way as venous blood—it has an impulse.

Now, examine the other patient. Here the symptoms are directly contrary to the former. The limbs are stiff and rigid; the pulse extremely high; the heart's action excited; the heat of the body not only in the extremities higher than is usual, but absolutely higher than the temperature of the blood in health in man.

Now, what is the explanation of these two cases? It is found by experiments performed on animals. In the first of the two cases there is irritation—perhaps extremely slight, the slightest prick will be sufficient—on the spinal cord. The effect is, stoppage of the heart's action, so that it beats with less force and rapidity, and, as consequences of this condition of the heart, all the other symptoms above described ensue. In the other patient, on the contrary, the spinal cord has been cut across, and the patient is in a much worse state, in reality, than the other; still he seems to be far more alive than the other. He seems to have the power of reaction which we wish to find in patients; yet the danger of his position is far greater—in fact, he is sure to die; while the other, by means of an operation (which was performed to-day upon a patient in this city), may survive.

I proceed to make some remarks on the production of symptoms of brain disease. As you well know, our view of the production of symptoms of brain disease—disease in the brain proper—is, that a disease there produces paralysis by striking the organs of the will, and that there is a paralysis of the will, at least for that part of the body which is paralysed—that, if other symptoms occur, such, for instance, as any form of insensibility, or any form of convulsions, the connexion of the part is altered or perturbed in some way, and owing to that alteration, this or that form of insensibility or convulsions will occur. All the symptoms of brain disease, at least all of them that I know of, either alone or united one with another, or grouped just as you may fancy to group them, may be due to simple reflex action. I have not time now to demonstrate this, but I shall demonstrate, I hope fully, that they are *not* owing to loss of function or pressure upon neighbouring parts.

You are all familiar with the great variety of symptoms presented in brain disease. Take, for instance, facial paralysis in cases of disease of the brain. Facial paralysis, as you know, does not exist in the orbicularis, but in the other muscles of the face. Now, if you say that in cases of disease of the anterior lobe of the brain (as is certainly the case) there is facial paralysis, because the nerve fibres of the facial nerve go to that part—well, I am perfectly willing to admit it; but let us take a case of injury to some other part of the brain—take a case of injury in the posterior lobe, and here, too, we find facial paralysis. How will you explain this? Do the nerve fibres of the facial nerve go to the anterior lobe in one and to the posterior in another individual? Certainly not. The result of such a hypothesis would be, that there is absolutely no part of the brain which would not be the spot to which the nerve fibres of the facial nerve go. If you imagine such a thing possible, I wish you would reconcile the facts with what anatomy teaches. Anatomy teaches that the facial nerve goes to a certain part of

the pons varolii, so that besides (excuse the word) the absurdity of supposing that the facial nerve extends to every part of the brain, and each part containing all those fibres—besides this absurdity there is the anatomical impossibility which we see when we examine the condition of the facial nerve. It is absolutely impossible, in fact, that such a hypothesis can be correct. Again, take another instance; the tongue, as you well know, is more or less paralysed in most of these cases of disease of the cerebellum: there is some difficulty in drawing it out in a straight line, also some slight impediment in the speech, owing to the paralysis of some of the fibres of the ninth pair of nerves. How can you reconcile the existence of that paralysis in most cases with the fact that we do not see the fibres of the ninth pair of nerves—the hypoglossal—going up higher than the medulla oblongata? Here is a patient whose pons varolii is completely injured—mark that the pons varolii must be the place of passage—if there is any such passage—of the fibres of the hypoglossal pair going up to the brain. If, therefore, one-half of the pons varolii is congested by disease there must be a paralysis of the tongue, yet there is no paralysis of the tongue in that case, so that both anatomy and this physiological fact prove that the hypoglossal nerves do not go to the back of the brain. How, then, is it that disease in *any* part of the brain may (as we know it does by experience) produce paralysis of the hypoglossal pair? I leave it to you to decide. To me it seems quite clear that to hold that when an injury to a part of the cerebellum causes paralysis of the ninth, or any other nerve, it is because the paralysed nerve goes to that part of the brain, is decidedly wrong, and that there is no way of explaining the phenomena without admitting that irritation, starting from any part whatever of the body, may cause paralysis of any other part—*e.g.*, that irritation of any part of the bowels might produce paralysis of the tongue. If you examine a number of cases of disease of the brain, especially of the active form, such as cases of tumours producing irritation, and especially tumours in the membranes of the brain, you will find that, for a tumour in one and the same part of the brain, there is no symptom produced in some cases, while in others you will find any symptom whatever. I do not think you could put your finger on any form of nervous complaint that you will not find to exist in some one or other of the cases of injury in any part of the lobe of the brain. Still more, with the same persistent disease you may have during the course of the life of the patient, whether he is to recover or die, you may have a great change. The patient may be paralysed to-day, another day he may not; you may, in fact, have every variety of phenomena, or no phenomena at all,—all arising from one and the same cause, and in the same individual, so that unless you suppose that each individual part of the brain possesses every function whatever, and has the effect of reacting on every part of the body in a direct way, —unless you prove that each part of the brain is connected with the fibres of the whole body, you cannot understand these facts; and mark, in some cases of injury to the cerebellum, there are no phenomena at all, so that while, on the one hand, you must suppose

that there is no part of the brain which does not contain all the nerves of the body, you are, on the other hand, forced to conclude, that there is no part of any nerve of the body going to the cerebellum. Such a hypothesis is obviously impossible. You are aware that the explanation I have ventured to offer of these phenomena is, that they come under the class of those produced by reflex action. I shall present a few other instances of a similar kind; and first, a few words as to syncope, as induced by a blow on the stomach, which is nothing but syncope by reflex action. In these cases experiments have shown that the syncope is produced by reflex action through the abdominal sympathetic ganglion acting through the par vagum, medulla oblongata, and spinal cord. I have often and often tried the experiment by crushing the ganglion of the sympathetic in the abdomen. In such cases there was sometimes a sudden arrest of the heart's action, in other cases only a temporary diminution in the beating of the heart, in other cases, again, there was hardly any effect produced. Again, in those animals in which I observed the effect to be produced, I waited till recovery was established, and I then divided the ganglion and crushed the par vagum; no effect was then produced on the heart's action, clearly showing that the transmission takes place through the par vagum.

In those cases in which the heart is stopped, whether from the cause above assigned or any other,—I mean cases of syncope, I do not mean death, when of course the stoppage of the heart's action is a definitive one,—but those conditions which are on the verge of death, and which lead to death if nothing is done to relieve it,—in those conditions there very frequently are means of restoring life. I have ascertained in animals very frequently that though the heart is then quite stopped, we can, by simply pressing on the sternum, and by giving a hard push to the heart, make it beat. It will not beat long if the cause of the syncope is a powerful one; but beat it will, and if you continue the cause of irritation it will continue to beat, and in that way you may often revive the patient. But this is not all. If you add to that cause of revival another which is most powerful, and which is directly the reverse of what John Hunter did upon himself when he found he was in a state of syncope one day at college,—if, instead of breathing as quickly as you can, you stop the patient's breathing altogether, just as if you were trying to kill him by suffocation, you revive him, by producing a state of asphyxia, the patient is saved, he will have a struggle, and will come out of it very quickly. Nothing, indeed, is more powerful to make the heart beat than an accumulation of carbonic acid in the blood. Whether I have been right or wrong in maintaining the principle, that the normal and abnormal beatings of the heart when very tumultuous depend chiefly upon an accumulation of carbonic acid in the blood,—whether I am right in this respect or not, there is no question that if you produce partial suffocation in these cases, you make the heart beat again, and beat with force. I should add that I have not the merit of having discovered this fact, as I find that in an old book, published some two hundred or three hundred years ago, an English surgeon has mentioned this fact as very important.

He, however, does not say on what he grounded his view. There are some other features about syncope of great importance. If there is little blood circulating you may in a moment throw something like one or even two pounds of blood into the heart by simply pressing on the four main arteries of the body. If you press those four arteries you prevent circulation going on in them, and at once an immense quantity of blood returns from the venous system to the trunk, and there is an immediate revival.

A few words now upon asphyxia. There are experiments which show, as clearly as possible, that if you take two animals, one of them having had its temperature very much diminished, the other at a normal temperature; dip them both into water at the same time; the one having its temperature very low will survive the other twice, three times, and sometimes even five times; the duration of life under water being extended sometimes to twelve or fifteen minutes. The greater the previous diminution of temperature, the longer the duration of life. There is another fact which is a very interesting one. It is well known that persons who have fallen into very cold water have in many cases been drawn out and revived after a number of minutes' immersion. Now, in experiments performed upon animals by applying galvanism to the part, so as to stop the heart's action, which is just the effect a fall into water will produce, we find life will last much longer, the animal will be able to survive a much longer stoppage of the heart's action from having had an attack of syncope just before the asphyxia. This case, then, is exactly the reverse of the former. In one, syncope was cured by asphyxia; in the other, asphyxia is less mortal, because syncope previously existed.

In cases of death by asphyxia, if the temperature is low, there is one fact very similar to what we see in cases of sudden emotion producing arrest of the heart's action when the individual falls into cold water, and that is, that in the two cases—diminution of temperature, or dipping into water—the heart beats very much slower. Any patient attacked with asphyxia, whose temperature is much diminished, has a slow beating of the heart, and whose blood is red, is not exactly an asphyxiated patient; there is a mixture of syncope and asphyxia, and the patient has much more chance to recover. If you try to raise the temperature of such a patient, you run the risk of killing him.

I shall now say a word upon poisoning. Poisoning is often the cause of death by producing such diminution of temperature as is incompatible with life. Take, for instance, two animals which have been poisoned with the same quantity of opium. Supposing the temperature to be cold in the room, lay them both on a table, one covered carefully with warm clothes, the other exposed to the cold; you find, *ceteris paribus*, that the one which is kept warm will survive, while the other will die. This fact we find with almost every poison of an organic nature, that there is considerable diminution of temperature produced, if not *per se* sufficient to cause death, enough, at any rate, to add a powerful cause to the other causes existing. Now this diminution of temperature is a feature

which we can fight against, and it is therefore of the utmost importance in cases of poisoning to use every means to keep up the temperature of the body.

I am now obliged to stop this exposition. My object was to show that by the knowledge derived from experiments on animals, from pure physiology, as well as from the knowledge derived from other researches than those made on animals, the knowledge we derive from microscopic anatomy, and even from simple descriptive anatomy, as regards the base of the brain especially—from all these sources, combined with the study of pathological cases at the bedside—from all these facts we can indeed advance with great rapidity, and we are enabled to form a sure diagnosis in many otherwise obscure cases. I may illustrate the importance of the knowledge of physiology, especially of the advances made in the science of late years, by the successful practice of many men in this city and in England; I shall not attempt to mention names, as no doubt the names of the men to whom I allude are present to your memory. To follow in the footsteps of these men, doing what they have done, and perhaps giving more time than they have given to the study of physiology, will give to you the greatest help both in your diagnosis and prognosis; and, what will, perhaps, prove a still greater comfort to you, your conscience will always be at ease when you meet a complicated case. I hope that, as I have been speaking to many men of eminence—men much more advanced than I am in life, in knowledge, and in practice—I hope what I have said will not hurt their feelings; and that they will find an apology for me in the fact that, as I have been a physiologist before becoming a practitioner, I have been able perhaps to find more than most of you the advantage of that science. I hope, therefore, there will be no offence in my conclusion.

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